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## Civil Engineering.

*Memoir upon the Stability of Revetments, and of their Foundations.* By M. PONCELET, *Chef de Bataillon du Génie. Translated from "No.* 13 *du Mémorial de l'Officier du Génie," by Captain* JOHN SANDERS, *Corps of Engineers.*

[CONTINUED FROM PAGE 79.]

CHAPTER II.—*Vertical Revetments, considered on the Hypothesis of Sliding.*

42. The fact, mentioned at the end of the last chapter, as established by the discussion of Col. Audoy, is such as to become important under certain circumstances. The principal case arising will be in retaining walls at the foot of high embankments, or deep cuts. We therefore consider that it will be useful to go over the calculations of this learned engineer, and to carry our investigations to such an extent as to admit the least possible error, or uncertainty, in the determination of the coefficient of stability relative to the sliding of revetments on the beds of any of the courses of masonry.

*Exposition of Formulas, and Formation of Tables of Thicknesses, relative to this case.*

43. Continuing the notation adopted in numbers 1, 15 and 26, we shall, in the case where the back of the wall is vertical, have

$$(y) \qquad f^2\,(x-m)^2+2\frac{p'}{p}\,f\,x=\delta'\frac{f}{f'}(\sqrt{1+f^2}-\sqrt{u^2+f^2})^2(1+a)^2$$

to express the conditions of equilibrium of stability relative to sliding on the foundation AB (fig. 1) of the wall; in which equation we shall always have (26)

$$u=\frac{a-f(x-m)}{a+1}, \text{ or } a+1=\frac{1+f(x-m)}{1-u}$$

with the particular condition of $a>f(x-m)$, or of $u$ being positive. In substituting for $(a+1)$ its value in $x$ and $u$, the equation $(y)$ can be put under the form:

$$(z)\qquad \frac{pf^2(x-m)^2+2p'fx}{p[1+f(x-m)]^2}=\delta'\frac{f}{f'}\frac{(1+u)^2}{(\sqrt{1+f^2}+\sqrt{u^2+f^2})^2}.$$

44. For small loads (fig. 2), where the condition $a<f(x-m)$ is, on the contrary, satisfied, we shall simply have, (9) in observing that $n=0$,

$$\text{tang. }\theta=\text{tang. }\alpha=\frac{1}{f}, \text{ tang. }\tfrac{1}{2}\alpha=\sqrt{1+f^2}-f, \text{ \&c.,}$$

$$x=p\frac{\delta'f(\sqrt{1+f^2}-f)^2(a+1)^2+f'a(a+2fm)}{2ff'(p'+ap)}.$$

45. It will be recollected, on the other hand, that, if the width of the base CI (fig. 1) of the prism of earth which covers the summit of the wall, is given, it will then be necessary to make use of the next to the last of the equations in number 11, which will allow us to calculate directly the value of $x$, or of $e$, without recurring to the solution of equation $(z)$.

46. Also, on the supposition of $m=x$, $u=\frac{a}{a+1}$, or in the particular case when the berm equals the width of the wall, the equation $(z)$ immediately gives,

$$x=\frac{\delta'p}{2f'p'}\frac{(2a+1)^2}{[(a+1)\sqrt{1+f^2}+\sqrt{a^2+f^2(a+1)^2}]^2},$$

for all values of $a$ from zero to infinity. The latter value gives the formula:

$$x=\frac{\delta'p}{2f'p'(1+f^2)},$$

for calculating the finite limit of the thicknesses of vertical revetments, relative to the particular case under consideration.

47. The equation $(y)$ or $(z)$ generally being of the fourth degree in $a$ and $x$, it will be necessary to resolve it by methods of approximation analogous to those of the 31st, and following, numbers, for either deducing the root applicable to any special case, or making regular tables of thicknesses of demi-revetments, relative to different loads of earth and to various hypotheses in regard to the values of $m, f, p$, and $p'$.

Moreover, the formation of these tables will be much facilitated, because the equation ($z$) is explicitly of the second degree only in $x$, and, therefore, allows us to calculate directly this quantity for each of the values given to $u$, and, consequently, to deduce from it the corresponding value of $a$.

Supposing, for abridgment,

$$\frac{(1+u)^2}{(\sqrt{1+f^2}+\sqrt{u^2+f^2})^2}=V^2, \quad \delta'\frac{f}{f'}V^2=q,$$

and putting equation ($z$) under the form,

$$(w) \quad (q-1)f^2(x-m)^2+2\left(q-\frac{p'}{p}\right)f(x-m)+q-2\frac{p'}{p}fm=0,$$

we at once deduce from it, by resolving it with reference to $f(x-m)$,

$$x=m-\frac{pq-p'}{f(pq-p)}\pm\frac{1}{f}\sqrt{\left(\frac{pq-p'}{pq-p}\right)^2-\frac{pq-2p'fm}{pq-p}},$$

giving double roots, both of which may, at the same time, be positive, and may thus, in general, lead to two systems of simultaneous values of $x$ and $a$, running in contrary directions, but related to a common value,

$$x=m-\frac{pq-p'}{f(pq-p)},$$

which is the result when the radical vanishes, that is, when we have chosen such a value for $u$ as to give the relation,

$$\left(\frac{pq-p'}{pq-p}\right)^2-\frac{pq-2p'fm}{pq-p}=0, \text{ or } q=1+\frac{(p'-p)^2}{2pp'(1-fm)-p^2}$$

which particular value we shall name $q'$. It will be observed, when $q'$ exceeds unity, we shall have,

$$2pp'(1-fm)>p^2, \text{ or } m<\frac{p'-\frac{1}{2}p}{fp'},$$

for all applicable cases.

Whereas, for any greater value of $u$ than the one giving $q'$, we shall, from the nature of the function V, which always increases with $u$, from $u=0$ to $u=1$, have

$$q>1+\frac{(p'-p)^2}{2pp'(1-fm)-p^2}, \text{ or } \left(\frac{pq-p'}{pq-p}\right)^2-\frac{pq-2p'fm}{pq-p}<0.$$

Consequently, the values of $x$ and $a$ becoming imaginary, it would be useless to continue the substitutions of $u$.

48. Moreover, this limit of $u$ corresponding to $q'$, and which we shall likewise call $u'$, can be obtained directly by an equation of the second degree,

$$\frac{1+u'}{\sqrt{1+f^2}+\sqrt{u'^2+f^2}}=V'=\sqrt{\frac{q'f'}{\delta'f}},$$

which gives, neglecting the negative value $u' = -1$, a value entirely foreign to the equation,

$$u' = \frac{2\,V'\sqrt{1+f^2} - 1 - V'^2}{1 - V'^2},$$

an expression, however, which can only be adopted when its value will be below unity, and when that of $V' = \sqrt{\frac{q'f'}{\delta' f'}}$ is not greater than the ratio of 1 to $\sqrt{1+f^2}$; for the substitutions must cease with the value of $u = 1$, which renders $a$ infinite.

49. This discussion will serve in a great degree to diminish the necessary calculations in preparing the auxiliary table of the values of $x$ and $a$, which is the table mentioned in article 31; it will also permit us to discover, *a priori*, the law followed in each case by the first of these values, and particularly to detect, for the general case, the conditions under which the thickness of $x$ is susceptible of a finite limit, even when the height of the load converges towards infinity.

The quantities $p'$, $p$, $f$, and $m$, always preserving finite values, we see by the general expression for $x$ (47), that it can only take the form of $\frac{1}{0}$ for the single positive value of $u$, which renders $pq - p$ nothing, or $q$ precisely equal to unity, that is to say,

$$V^2 = \frac{f'}{\delta' f}, \text{ or } V = \sqrt{\frac{f'}{\delta' f}}.$$

50. This same value of $u$ necessarily being under the limit considered in article 48, it is evident that the substitutions should not cease with it; but as, in its passage through infinity, $x$ will have changed its sign, one of the roots of the equation ($w$) will no longer represent values of the thicknesses sought for, whilst the other will continue to give appropriate and positive values of $x$. Moreover, we can easily take this circumstance into consideration, by observing that the condition, $q = 1$, will only render one of the roots of this equation infinite. Its term in $(x - m)^2$ disappearing, if $x$ is finite, the equation will simply give

$$x = \frac{p(1 - 2fm)}{2f(p' - p)},$$

for the other root, which will, in effect, preserve a finite and positive value as long as $p'$ exceeds $p$, and $m$ is not greater than $\frac{1}{2f}$.

### *Discussion relative to the limit of thickness, and to the influence of the choice of a coefficient of stability.*

51. Let us return to the general question, and investigate what is the condition in order that the thickness of the wall may preserve a

finite limit, even when the height of the load converges towards infinity. We have seen (49) that the value of $x$ only takes the form $\frac{1}{0}$ when the quantity $q$, or $\delta' \frac{f}{f'} V^2$, passes through unity in the interval comprised between $u=0$ and $u=1$, which last value of $u$, in all cases, answers to an infinite load. Now, the function

$$V^2 = \left( \frac{1+u}{\sqrt{1+f^2}+\sqrt{u^2+f^2}} \right)^2,$$

increasing in a continuous manner in this interval, and its greatest possible value answering to $u=1$, for which it simply becomes

$$V^2 = \frac{1}{1+f^2}, \text{ which gives } q = \frac{\delta'}{f'} \frac{f}{1+f^2},$$

it results from it (47) that the thickness of a revetment will necessarily preserve a finite limit, if this greatest value of $q$ should be below unity; that is to say, in all cases where we shall have

$$\frac{\delta'}{f'} < \frac{1+f^2}{f},$$

but the minimum of the expression $\frac{1+f^2}{f}$, corresponds to the case of $f=1$, when the natural slope of the earth is 45°, and its value is then reduced to 2. In fine, the thickness of a revetment will preserve a finite limit in all cases arising in practice, if we constantly have

$$\delta' < 2f', \text{ or at the most } \delta' = 2f',$$

and when it is otherwise, the thickness will converge towards infinity, at the same time with the height of the load.

52. We perceive what an important influence the choice of a coefficient of stability may exercise in the actual case upon the determination of the thicknesses of revetments, relative to very great heights in the superincumbent earth, and how necessary it becomes to fix upon correct data in regard to it, if we do not wish to risk running into dimensions which would beyond measure exaggerate the expense, or which might seriously compromise the solidity of the work. In particular, the coefficient of stability $\delta' = 2.38$, which M. Audoy obtained by taking, after M. Boistard, $f' = 0.75$, this in general giving $\delta' = 3.17f'$, would lead us to thicknesses which would increase rapidly and indefinitely with the height of the load of earth. The same conclusions will, *a fortiori*, follow from the value which we shall obtain for $\delta'$, if we take into consideration the weight of the earth which acts upon the summit of the wall, and to which this officer paid no regard; for, in going over his calculations on this hypothesis, and in continuing to take the profile of Vauban as a point of comparison, and supposing (15): $f=1, p=\frac{2}{3}p'$, we shall find $\delta' = 3.3f'$, in the place of $3.17f'$, or $\delta' = 2.475$, in place of $\delta' = 2.38$.

We do not think that, in any case, such a number should be adopted for calculating the thickness of revetments, nor even 2.3, to which M. Audoy had definitely reduced the coefficient of stability, relative to sliding. We shall soon explain the motives for it; but, usage having in some degree given importance to the considerations which served to obtain it, we believe it proper to examine in itself the influence of the magnitude of the ratio $\delta' : f'$, upon the progress of the values of the thickness $e$, on the particular hypothesis of $f = 1$, $p = \frac{1}{3} p'$.

53. The maximum value of $\delta' : f'$, which allows these thicknesses to preserve constantly a finite value, being then equal to 2 (51), and that given by the ordinary revetment of Vauban being, as has just been said, 3.3, we have successively calculated (147) on the hypothesis in question, tables of these same thicknesses relative to the values $\delta' = 2f'$, $\delta' = 3.3f'$, which we shall consider as limits, and to their arithmetical mean $\delta' = 2.65f'$, so as to be able to form clear ideas upon the true influence of these essential given quantities on the question, and upon the limit of the heights of loads, which, in each case, will require greater thicknesses to those which answer to the hypothesis of rotation, and to a coefficient of stability, $\delta = 1.912$ (18.)

These partial results are found united and arranged for comparison in the following general table, where we have also introduced an extract from that of number 34, relative to this same hypothesis of rotation, as well as the thicknesses given by the formula:

$$e = (0.2\, a + 0.1)\, H + 1.225 \text{ metres},$$

which reproduces, for vertical walls and the case of sliding, the stability of the profile of Vauban, without counterforts, according to the principle of transformation which will be explained in the following chapter.

*Comparative Table*

of the thicknesses of Vertical Revetments supporting high superincumbent loads of earth, calculated for earth and masonry of mean densities, on the hypothesis either of rotation or of sliding, and in attributing different values to the coefficients of stability and friction, relative to the latter hypothesis.

| Values of $a=\frac{h}{H}$ | From the rule of Vauban. | On the hypothesis of the rotation, the berm being | | From the equations relative to sliding | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | 1st hypoth of $\delta'=3.3f'$, berm being | | 2d hypothesis of $\delta'=2.65$ $f'$, the berm being | | 3d hypoth. $\delta'=2f'$, berm being | | | | |
| | | Zero. | equal to 0.2 H | Zero | equal to thickn'ss $e$ | Zero | equal to 0.2H | equal to thickn'ss $e$ | Zero | equal to thickn'ss $e$ | | |
| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | |
| | | metres | | | | | | | | | | |
| 1.0 | 0.30H+1.23 | 0.61H | 0.54H | 0.41H | 0.57H | 0.39H | 0.42H | 0.37H | 0.31H | 0.31H | 0.23H | |
| 1.1 | 0.32H+1.23 | 0.63H | 0.56H | | 0.61H | | 0.45H | 0.40H | | 0.32H | | |
| 1.2 | 0.34H+1.23 | 0.65H | 0.57H | | 0.65H* | | 0.47H | 0.42H | | 0.34H | | |
| 1.3 | 0.36H+1.23 | 0.68H | 0.59H | | 0.68H | | 0.50H | 0.43H | | 0.35H | | |
| 1.4 | 0.38H+1.23 | 0.70H | 0.60H | 0.42H | 0.72H | 0.42H* | 0.52H | 0.45H | 0.33H | 0.36H | 0.25H | |
| 1.5 | 0.40H+1.23 | 0.72H | 0.61H | 0.42H | 0.75H | 0.42H | 0.55H | 0.46H | 0.34H | 0.37H | 0.26H | |
| 2.0 | 0.50H+1.23 | 0.80H | 0.66H | | 0.94H | | 0.68H | 0.54H | | 0.43H | | |
| 3.0 | 0.70H+1.23 | 0.89H | 0.72H | 0.44H | 1.35H | 0.48H | 0.87H | 0.68H | 0.58H | 0.51H | 0.29H | |
| 3.2 | 0.74H+1.23 | 0.91H | | | | | 0.91H* | | | | | |
| 3.5 | 0.80H+1.23 | | 0.74H | | | | | 0.75H* | | | | |
| 4.0 | 0.90H+1.23 | 0.96H | 0.76H | | 1.76H | | 1.04H | 0.81H | | 0.56H | | |
| 5.0 | 1.10H+1.23 | 1.00H | 0.78H | | 2.18H | | 1.20H | 0.93H | | 0.62H | | |
| 10.0 | 2.10H+1.23 | 1.11H | 0.84H | 0.45H | 4.44H | 0.52H | 1.92H | 1.45H | 0.42H | 0.75H | 0.32H | |
| 15.0 | 3.10H+1.23 | 1.15H | 0.86H | | 6.99H | | 2.95H | 2.36H | | 0.83H | | |
| 20.0 | 4.10H+1.23 | 1.17H | 0.88H | | 9.79H | | 4.90H | 4.11H | | 0.85H | | |
| 30.0 | 6.10H+1.23 | 1.19H | 0.89H | 0.46H | 16.24H | 0.54H | 8.94H | 8.80H | 0 43H | 0.90H | 0.33H | |
| 50.0 | 10.10H+1.23 | 1.23H | 0.92H | | 32.42H | | 17.35H | 17.30H | | 0.93H | | |
| Infin't | Infinite | 1.24H | 0.93H | 0.46H | Infinite | 0.55H | Infinite | Infinite | 0.44H | 1.00H | 0.33H | |

*Principal Consequences.*

54. Here, again, the length of the calculations has constrained us to leave incomplete the part of this table which relates to revetments with berms equal to a fifth of their height; but, for the particular object in view, it was sufficient, as will be seen, to treat carefully the case where the ratio of $\delta'$ to $f'$ attains its mean value 2.65, and which shows that the berm may, in the case of heavy loads, exercise a very appreciable influence in diminishing the thicknesses of revetments, as also happens on the hypothesis of rotation.

In effect, if we at first compare the numbers of the eleventh and twelfth columns, relative to $2f'$, the smallest of the values attributed

to $\delta'$ in the case of sliding, with those which respectively correspond to them in the third and fifth columns, relative to rotation, and to 1.912 as a coefficient of stability, we at once perceive that the thicknesses for all widths of berm, calculated on the first hypothesis, are constantly less than those obtained on the second. It is to the latter, then, we should only have regard, particularly if it is ordinary earth and masonry in question. We are, besides, assured by the calculation, that the same circumstances are reproduced for all values of $p'$ greater than that of $\frac{3}{4}p$, which answers to the mean case.

If, then, we compare the sixth and seventh columns, relative to 3.3 $f'$, the greatest value of $\delta'$, with the third and fifth, their corresponding ones, which refer to rupture by rotation, we shall see, by the asterisks which designate certain results in the sixth and seventh columns, what are, for each case, or each width of berm, the heights of loads, beyond which it becomes necessary, in the mean case, to substitute the hypothesis of sliding for that of rotation.

In fact, the asterisks of the eighth and ninth columns, relative to the mean value of the ratio $\delta' : f'$, show that this limit of the heights of the load is quite remote; and the want of any asterisk in the tenth column proves, as we had at first remarked, that this same limit does not exist for a vertical revetment with a berm precisely equal to its thickness; but it is necessary not to lose sight of the fact that these relations have only been investigated for earth and masonry of mean density and cohesion.

55. Let us now undertake the comparison of the thicknesses given by the rule of Vauban, which are to be found in the second column of the table, with those obtained on the hypotheses of $\delta' = 3.3f'$, $\delta' = 2.65f'$, $\delta' = 2f'$, which we have just been discussing.

And let us, beforehand, remark, that the first of these thicknesses, which are deduced from a formula of transformation (53) specially referring to the case of sliding, are, for equal values of $a$, respectively less than their corresponding ones in the second column of the table inserted at number 20, and which result from another formula of transformation relative to the case of rotation; a circumstance which, it may be remarked in passing, justifies the usage, which generally obtains, of only considering this last method of the transformation of the profile of Vauban, since it is the most favorable to solidity.

The constant terms of these same formulas, moreover, rendering impossible all direct and absolute comparison between the results which they furnish, and those of the sixth and twelfth columns of the foregoing table, it will suffice, for the object we have in view, to remark:

1°. That, excepting for demi-revetments, the height H of which is less than four or five metres, the thicknesses (sixth column) deduced from the equation (z) on the hypothesis of $\delta' = 3.3f'$, generally surpass, very much, those of the second column, as given by the rule of Vauban, and also converge towards infinity in a much more rapid manner.

2°. That the thicknesses (eighth and ninth columns) relative to the

hypotheses $\delta' = 2.65 f'$, approximate much closer to it, particularly for very great values of $a$.

3d. That, in fine, the hypothesis of $\delta' = 2 f'$ (eleventh column) always leads to a less thickness than that given by this same rule.

56. It seems to result clearly from this approximation, that the value 3.3 of the ratio $\delta' : f'$ being that of the pressure to the weight of the wall, together with its superincumbent load, and, consequently, that the coefficient of stability 2.50, which is deduced from it (52) on the hypothesis of sliding, leading to useless exaggerations of thickness, should not be adopted in practice, although they so correctly answer to the particular revetment of Vauban, of ten metres high, covered by an earthen parapet two metres in height. Demi-revetments, established according to the rule of this illustrious engineer, have, in a great number of cases, undergone the test of experience, and constructors have had as much fault to find with the excess of thickness which it furnishes for great loads of earth, or low walls of masonry, as they have had to complain of their insufficiency in precisely contrary cases.

In the second place, the evident harmony which exists between the thicknesses furnished by the rule of Vauban, and the corresponding ones of the eighth and ninth columns of the table, would seem to cause the adoption of 2.65 for the value of the ratio $\delta' : f'$, as it is about a mean of the values, and corresponds to a coefficient of stability $\delta'$ nearly $= 2$, if we take (52), $f' = 0.75$.

But, to confine ourselves invariably for coefficients to such figures, considered as the inferior limits of those which give a sufficient preponderance to the resistance of a revetment against the action of the earth, it would be necessary to be certain that the thicknesses furnished by the rule of Vauban are not themselves exaggerated, for the case of $f = 1$ and $p' = 1.5 p$; and it would be necessary to be better informed upon the true influence exercised by the friction and cohesion of masonry at the first instant of its construction.

(To be continued.)

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FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

## *On the Friction Dynamometer, or Brake, of M. de Prony, a cheap, simple, and effectual instrument, for measuring the actual power developed by machines.* By Ellwood Morris, C. E.

It is well known that *friction*, in all machines, consumes or wastes, more or less of the motive power, and that it may, at will, be so augmented, as to consume the whole power of a prime mover; thus by gearing to any steam engine, or other motor, a train of wheel work of sufficient dimensions, and extent, it would be possible, so to multiply the friction of the moving parts, as to use all the power developed by the motor, and leave none for application to useful purposes.

This must be so evident upon reflection, that further illustration seems unnecessary, and it follows, consequently, that in any case where the friction produced, consumes the whole power of the ma-

chine; if the amount of that friction could be, by any process, correctly ascertained, it would furnish an accurate measure of the power of that machine; *for the retarding force, which consumes the power of a motor, must be an accurate meter of that power.*

M. de Prony, a very distinguished French Engineer, (in the year 1821) was the first to realize, from similar reasoning, and to put in use, a cheap and simple instrument, which should at once, *consume the power of a motor, and measure the power so consumed.*

The simple form of the *Brake*, originally devised by M. de Prony as a Dynamometer, has been occasionally modified by other engineers, to suit the particular circumstances of the motors, by them subjected to experiment.

Capt. Arthur Morin, of the French Artillery, (a mechanical philosopher who is fast acquiring a well deserved celebrity) has given a lucid description of the Brake, or friction Dynamometer, used by him in an admirable series of experiments made between the years 1828 and 1835, upon water wheels of large size, then actually driving various heavy works in France.

A detailed account of these experiments was published at Metz and Paris, in 1836, under the title of "*Expériences sur les Roues Hydrauliques a aubes planes et sur les Roues Hydrauliques a augets.*"

M. Morin in that publication, has briefly but explicitly, developed the theory of the "*Frein Dynamométrique*," or *Brake Dynamometer* of M. de Prony, and has also given some particular directions for its use; this part of the work referred to, we have in substance translated below, for the information of our readers, and we shall follow it, with some observations, relative to a recent application of Prony's Brake, to measure the power of a motor in this country.

### *Remarks of M. Morin, on the Brake, or Friction Dynamometer, of M. de Prony.*

[TRANSLATED.]

#### I. *Description of the Brake with a movable ring.*

"The construction and arrangement of the Brake dynamometer, which M. de Prony first applied to measure the useful effect of motors, is well known to mechanicians; but I believe, I ought nevertheless, to describe briefly that which I lately used, and of which I have borrowed the chief forms, from a German Engineer, M. Egen, who has published on the same subject, a collection of very interesting experiments.*

The apparatus is composed of an annular collar of cast iron, (see the annexed figures, 1 and 2;) this collar is in two parts united at *b*, *b*, by ears, with bolts and screws.

The inner diameter of this collar is $31\frac{49}{100}$ inches, which allows it to be placed on the large shafts of wheels; its thickness in the middle,

* Experiments made upon the Hydraulic Machines of Westphalia, by order of the Prussian Government, in 1828 and 1829, and published at Berlin in the German language, in 1831.—TR.

and on a breadth of $6\frac{30}{100}$ inches, is $1\frac{19}{100}$ inches; but practice has shown me that this was, perhaps, insufficient, as I will mention further on.

Fig. 1.

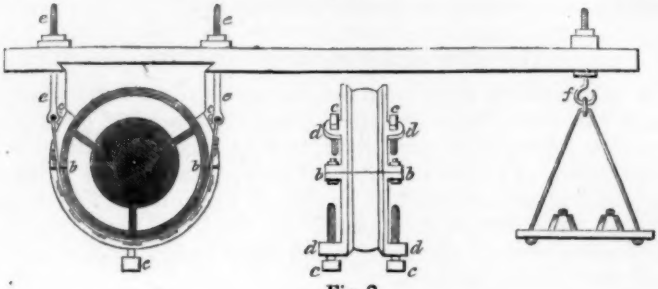


Fig. 2.

On the sides the collar is strengthened by a rim or flanch of $1\frac{19}{100}$ inches projection; intended to render it more rigid, and to hinder the friction pieces from escaping laterally."

II. *Manner of centering the ring or collar of the Brake.*

"The outer surface of the throat was turned with care, so that it answered to centre it relatively to the shaft on which we set the collar, in order to obtain a cylindrical surface exactly concentric to this shaft. It is for rendering this operation certain, and easy, that six large screws, *c c c*, square on the head, are symmetrically disposed on the exterior of the ring, and traverse ears *d d d d*, which serve them to screw by.

The ring being established, and joined on the shaft, we see that by suitably manœuvring the centering screws, it will be very easy to adjust the outer surface. This operation, being very important for the success of the experiments, it is indispensable to make it with the greatest care.

The screws, *c c c*, have about 10 inches of their length filleted, or cut with a thread, which answers for most cases; nevertheless if we wish to operate on a shaft of less than 17 inches in diameter, they will be too short; but we remedy this inconvenience by making the screws to press on intermediate wedges of sufficient thickness. This has no reference to shafts of cast or wrought iron, on which we cannot fix this apparatus without mounting it previously, on a cylindrical or prismatic tube of sufficient size.

We shall be able therefore easily to centre the collar, but the effort which tends to make it turn, with the proper motion about the shaft, being frequently very great, the screws would be likely to be bent, or to furrow the surface of the shaft if it were of wood; to avoid these inconveniences, it is necessary, after the centering, to wedge up strongly, the collar on the shaft, by the aid of wedges, disposed, two and two, so that their outer faces, may be always parallel to the axis.

Three or four pairs of wedges thus arranged, and suitably fastened, suffice for fixing the collar solidly, but it is necessary to take care

and strike little by little, and turn by turn, in order not to make the annular surface take an eccentric curvature; this will require, I believe, that this part should have a greater thickness than that of $1\frac{19}{100}$ inches which I have given it, and which nevertheless has always been sufficient by taking the proper precautions."

### III. *Articulated chain of Pressure.*

"The collar being thus mounted concentrically on the shaft, we surround it by an articulated friction band, composed of eight plates of sheet iron $\frac{19}{100}$ths. of an inch thick, by $3\frac{94}{100}$ inches broad, joined or hinged, together, by bolts of $\frac{19}{100}$ to $\frac{24}{100}$ths of an inch in diameter; and the chain is curved to follow a radius of curvature, a little greater than that of the collar, to the end that the angles of the articulations may be able to receive the grease and other bodies, which may get between the rubbing surfaces.

By this arrangement, we obtain on the surface of the collar, a more systematic division of the pressure, than with a simple band of strong sheet iron; the articulated chain is terminated by two half links strengthened in the upper part, and forming the females of the hinge, for receiving the two plates, and bored parts of two large bolts, *e e*, of $23\frac{6}{10}$ inches long, by $1\frac{19}{100}$ inches in diameter, to which they are joined by small bolts of $\frac{59}{100}$ths of an inch in diameter.

### IV. *Lever of the Brake.*

"The bolts *e e*, traverse perpendicularly a piece of pine of $5\frac{19}{100}$ to $7\frac{87}{100}$ths inches square, in the largest of the upper part, (according to the power of the motor examined,) which piece forms *the arm of the lever of the Brake;* the bolts *e e*, are furnished with screws, and large nuts. The under side of the arm of the lever of the brake, receives, by notching, a cushion of *hard wood*, which rests on the collar by a cylindric part concentric to the surface; one or more holes pierced through the lever and the cushion, permits the pouring of oil for lubricating the surface of the collar.

At the extremity of the lever is a superior hook *f*, for a scale, or box, where we place the weights, which form the load of the brake.

It is proper that this hook should have two screws, the one above, the other below, or one screw above, and the projection below, between which the lever may be fastened, so that during shocks, the suspension may not be deranged.

The essential parts of this brake are composed, then, of *the collar, the chain, the bolts, the cushion, the hook*, and *a key or wrench*, for fastening the screws, the whole weighs no more than 440 to 550 lbs.

This apparatus is therefore sufficiently portable, so that a constructor of machines, has in it that which will enable him to measure the power of established motors, or to tell that of those which he has to put at work.

It seems to me very desirable that its use should become familiar to all practical men, for it will furnish them, on the one hand, with certain data for the establishment of projected works, and on the

other, it will certainly avoid disputes, concerning the effects of motors."

### V. *Mode of using the Brake Dynamometer.*

"To insure the success of the experiments, and to avoid the dangers that they may offer, induces me to give some instructions concerning the mode of making use of the brake dynamometer.

It is proper at first to examine if the wheel on which we wish to operate, is centred relative to the external form, and to the coincidence of its centre of gravity, with its axis of rotation.

It will be necessary then, to restore the buckets to a good state, and cause them to have the same play in relation to the sweeps of arrival and escape, and to the walls; then if the wheel is not in equilibrium about its axis, which we may easily observe, we add counterweights interiorly on the proper side to re-establish the equilibrium.

That done, we will visit the gudgeons and cushions, we will grease them properly, and we satisfy ourselves, that there is no friction from projections against the ends of the shafts or of the gudgeons.

The brake being adjusted as we have mentioned, we will place the lever in a horizontal position, then we will arrange in front and rear of the shaft, the beams or points of support, which in allowing it above and below this position, a play of 2 or 3 degrees, limits the oscillations in an invariable manner; this disposition which is much preferable to the ropes, or detaining chains, sometimes employed with the same design, will avoid all the dangers which might be occasioned by the accidental increase of the friction of the articulated chain, and of the collar, in consequence of which the lever would be raised up, and tend to be dragged around with its load, in the general motion of the wheel.

It has other advantages, in giving to the experiments sufficient precision, if we consider the lever of the brake, as being really in equilibrium, when it oscillates lightly between its two supports.

It is necessary, moreover, to be satisfied that the inertia of the masses in motion does not develope, during the continuance of the experiments, a power sufficiently great to make a sensible impression on the results; and we will arrive at it by counting, at many trials, the length of time necessary for a certain number of revolutions. When this shall be constant, we will be certain that the motion is uniform, or, at the least, regular, and that, for the interval considered, the total quantity of work developed by the inertia must be null.

The apparatus being mounted, the experiments are easily cast, and in very little time; we ought always to profit by this facility to make numerous series, corresponding to the different openings of gate, and heads of water, under which the machine can act.

Finally, in each series it will be proper to make the load vary progressively from zero, or the proper weight of the brake, in relation to the distance of the point of suspension, up to that which stops the machine, or, at the least, as near to this last as we can, without danger.

We shall easily determine, also, by experiment, the velocity under which the motor acts the most advantageously."

### VI. *Limits of the power which the Brake can equilibrate.*

"The dimensions of the brake which I have described above, are such that it could be applied to measure the useful effect of a great number of motors. Nevertheless, as the pressure of the chain, and, of course, the friction against the collar, ought to augment with the power of the machine, or the quantity of work measured, it attains, in some cases, a superior limit, that cannot be passed without the surfaces of contact abrading themselves; this, besides the inconvenience of injuring the apparatus, will have also that of depriving the results of the precision desirable, by producing shocks.

It will be proper, then, not to expose it thus, and, in cases where we shall have to measure the work of motors of great power, by placing the brake on shafts having a small velocity, we shall be able to determine the diameter of the collar, by the aid of the following observations.

In all the experiments where the brake described above was employed, I have always noticed that, in spite of the employment of unguents of oil, or hog's lard, the chain and the jaw abraded, whenever the friction at the circumference required to be from *one to one and a fifth tons, (or say one-fourth of a ton per inch of the breadth of the friction band,* Tr.) in order to equilibrate the force of the motor.

By estimating, then, about the maximum power that the motor can exercise under the most favorable circumstances, we shall be able easily to determine the radius of the jaw, so that the friction which will serve to measure this maximum power, never reaches the limit value that we have indicated above. How far we may deviate, is, moreover, evident by the following calculation, which establishes, at the same time, the theory of this apparatus."

### VII. *Theory of the Brake Dynamometer.*

"When the brake dynamometer is mounted and fixed, (on a water wheel shaft,) so that the lever and load shall be held in equilibrium, and oscillate lightly between the points of support, whilst under an opening of orifice, and head of water both constant, the wheel moves at a uniform velocity, it is evident *that all the available work, or power, transmitted to the wheel, is consumed by the friction of the articulated bridle chain against the collar;* and when we call

P′ the mean *disposable power* at the distance R from the axis of rotation,* which, in the case where the brake shall be mounted on the same shaft with the water-wheel, will be the outer radius of this wheel.

V, the velocity at the circumference R.

S, the friction which is produced at the surface of the collar.

*r*, the radius of this surface.

* This mean power P′ is evidently less than that which is transmitted to the extremity of the radius R, (and which we would designate by P,) on account of one part of it being employed to conquer the friction on the axis of rotation: this is the reason why I distinguish P′ by the name, *disposable power.*

Then we shall have, at each instant,

$$P' v = S \frac{r}{R} v, \text{ or } \qquad P' R = S r.$$

But, on the other hand, the lever of the brake, as well as its load, being maintained in equilibrium by the friction S, we have, in designating by

F, the total load of the brake;
L, the horizontal distance, from the point of suspension of this load, to the vertical plane of the axis of rotation;

then $F L = S r$, and, consequently, $P' v = F \frac{L}{R} v$.

But $\frac{L}{R} v$ is evidently the path that would be described by the point of suspension of the load in a second, if the lever moved with the shaft of the wheel.

We see, then, that *the product of the total load* F *of the brake, by the path that the point of suspension tends to describe in one second, measures the quantity of available power transmitted to the shaft on which we have the collar placed.*

We will remark, that, when we shall have estimated approximately, at its maximum value, relative to the most adverse case, the power P', and that the limiting value S shall be fixed at 1 to $1\frac{1}{2}$ tons, (for a brake of this size,) as we have seen above, it will be easy to determine the size of the radius $r$, to give to the collar of the brake, in terms of these quantities, and of the arm of the lever R of P'."

VIII. *Estimate of the power consumed in the passive resistances*

"The product $P' v = F \frac{L}{R} v$, which measures the quantity of power, or work, available, is, for the valuation of the useful effect of an established motor, the result most important to be known for industrial comparisons; but in those experiments where its action is to value the construction of a receiver of power, the effects of the action of the water, the influence of the velocity, and of the other circumstances of the motion of the machine, or when we have not been able to place the brake on the same shaft with the wheel, it is necessary, in order to obtain the total power realized by the receiver, to add to this quantity of work $P' v$, that which is consumed by the many pieces in motion. In effect, if, by considerations foreign to the mode of action of the water, we have been brought to give to these pieces considerable dimensions and weight, they may consume notable quantities of power, of which the value is entirely independent of the good or bad arrangement of the receiver, and which must absorb a part of that which it really *utilized*."

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Although it appears, from the above translation, that M. Morin, in developing the theory of the *brake dynamometer* by a general equa-

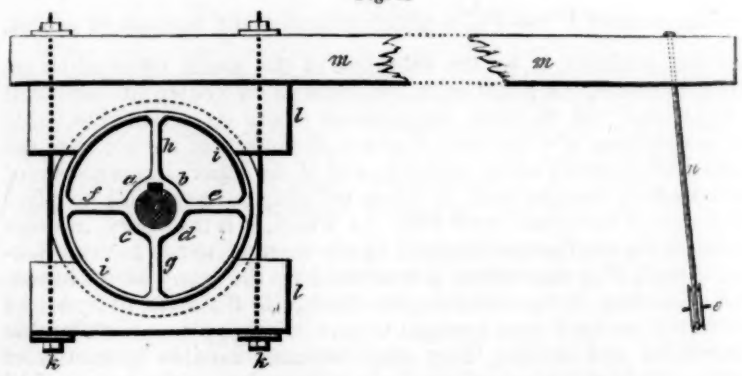
tion, found it necessary to employ algebra, still it is not requisite, in determining, practically, by the *brake*, the power of a motor, to resort, in the computations, to any other than the elementary rules of arithmetic; and it may, therefore, by observing certain simple rules, be correctly employed by any artisan, as we shall now proceed to show.

The writer having had occasion to measure the power of a *turbine* recently erected by Merrick & Towne, of this city, at the Rockland Mills, in Delaware, (and there applied to drive a cotton mill,) determined to use *Prony's brake* for that purpose.

One of these instruments was accordingly made at the Southwark Foundry, and consisted of a *turned cast-iron wheel, or pulley*, four inches broad upon the face, with a ring five-eighths of an inch thick, supported by four arms, and having a flanch about half an inch thick, and say one inch wide, projecting from one margin; the eye of this wheel was accurately bored out $4\frac{5}{16}$ inches in diameter, to fit the cast-iron upright shaft of the turbine, and a key seat being cut in it, opposite to an arm, to correspond to that designed to secure the bevel pinion, the brake pulley was keyed fast on the turbine shaft, in the place of the pinion. Two jaws of hard oak wood, connected with an arm, or lever, were then made to embrace this pulley partially, and, by means of nuts and screws, to press upon it, with any desirable degree of force; to the outer end of the arm, or lever, a cord was attached, which, being conducted in a proper direction over a small, fixed pulley, had suspended from it, by slender hooks, the weights which determined the power exerted by the turbine upon the *brake pulley*, and consumed by it in friction against the wooden jaws, or cushions.

A horizontal view of this arrangement is shown in the annexed sketch.

Fig. 3.




*Description of the Brake above sketched.*

*a, b, c, d*, the *eye* of the brake pulley, $1\frac{1}{2}$ inch thick.
*e, f, g, h*, the *arms* of " " 1 inch average thickness.
*i, i*, the *ring* of " " $\frac{5}{8}$ inch thick.

*k*, *k*, the *bolts*, with their nuts and screws, for tightening the friction jaws.

*l*, *l*, the *oak jaws*, 4 inches thick, and about 9 inches broad at the broadest, and 4 inches at the narrowest, part; these jaws were made to press upon the pulley by the screws aforesaid.

*m*, *m*, the *arm*, or *lever*, of the brake, about 5 by 6 inches in section, and near 13 feet extreme length.

*n*, the *cord* by which the weights were suspended, and the direction of which was perpendicular to a radial line 11 feet long.

*o*, the pulley over which the cord passed.

The shaded circle shows the shaft of the turbine, and the black spot, the key by which the brake pulley was fastened upon it.*

The turbine shaft being vertical, the brake pulley, its lever, &c., acted, of course, in a horizontal plane, and therefore it was that the pulley required merely a flanch projecting from the lower side, as gravity effectually countervailed any tendency of the friction pieces to rise up.

To relieve the brake pulley of the weight of the lever, it was suspended by its middle from the second floor of the mill, so as to vibrate freely in a horizontal plane.

When this brake was first applied by the writer, considerable difficulty was experienced in procuring regularity of friction; for, in consequence of neither the faces of the pulley, nor of the friction jaws, being perfectly smooth, a good deal of jarring took place.

This difficulty, however, was completely overcome, at the suggestion of another, by allowing the brake pulley to run under the action of oil and emery, and with a considerable friction from the jaws, until all the surfaces of contact were ground quite smooth.

The lever was allowed to vibrate in an arc of two or three degrees, between two stops firmly fixed, and, by touching lightly with a wrench, from time to time, *one* of the screws, there was no difficulty in causing it to maintain a central position, with sufficient steadiness.

Following the example of M. Poncelet and of M. Morin on similar occasions, we used *water* as a lubricator, and, while the brake was in action, one or two men with watering pots effectually prevented any burning of the jaws, and supplied a sufficient quantity of water to make the friction regular.

We now subjoin a few of the results obtained by the *brake*, in experimenting upon the abovementioned turbine, for the purpose of showing the simplicity of the mode in which the power developed by any motor tried, is computed by the aid of this dynamometer.

* This brake is upon the plan originally projected by M. de Prony. It was modified by M. Poncelet, in experimenting on his new form of the undershot wheel, in 1826, by substituting, instead of the lower cushion of wood, a broad friction band of wrought iron, tightened by two screws, and embracing *directly* the large wooden shaft of the water wheel. This disposition of the parts may sometimes be the best.

*Some results obtained with the Brake in trying the power of a Turbine.*

| No. of the experim's | No. of turns of wheel in a minute | Load of the brake in pounds, at the end of an effective lever, or radius of 11 feet. | Space in feet which the end of the effective lever, or radius of 11 feet, tended to describe in a minute. | Horses power of 33,000 lbs. lifted one foot high per minute; actually developed by the turbine. |
|---|---|---|---|---|
| 1 | 2 | 3 | 4 | 5 |
| 1 | 46 | 136 | $3178\frac{6}{10}$ | $13\frac{1}{10}$ |
| 2 | 52 | 122 | $3593\frac{2}{10}$ | $13\frac{3}{10}$ |
| 3 | 59 | 108 | $4076\frac{9}{10}$ | $13\frac{3}{10}$ |
| 4 | 64 | 98 | $4422\frac{4}{10}$ | $13\frac{1}{10}$ |

In this table, columns 2 and 3 are the data, whence 4 and 5 are easily calculated.

Thus, to obtain the results of column 4, we have given the number of turns made by the wheel in a minute, and the radius of the circle which the extremity of the effective lever of the brake (or perpendicular let fall from the centre upon the direction of the cord) would have described, if it had been free to revolve; this radius, in the above case, was exactly eleven feet in length.

Hence, to find the results which are placed in column 4, in the first experiment of the table, we have $(11 \times 2 \times 3.1416)^{\text{feet}} \times 46^{\text{turns}} = 3178\frac{6}{10}$ ft. And to find, in the same experiment, the horses' power developed, or the result in column 5, we have

$$\frac{3178\frac{6}{10}^{\text{feet}} \times 136^{\text{lbs.}}}{33{,}000} = 13\frac{1}{10}\ \text{HP.}$$

From these examples it is evident that, having given,

1. The number of revolutions of any motor in a minute, the brake being on the same shaft;
2. The weight in pounds suspended from the brake lever;
3. The acting length of that lever, or perpendicular distance from the centre of motion, to the line of action of the weight;

and having found the circumference of the circle which would be described in one free revolution of the extremity of the acting lever; then the number of horses' power developed by the motor, (or by the shaft on which the brake is mounted,) in any one experiment, or the useful effect of the machine, may be found by the following

RULE.

*Multiply together, successively, the load of the brake in pounds, the number of turns of the brake pulley in a minute, and the circumference of a circle of which the acting lever is the radius.*

***Divide this product by 33,000, and the quotient will be the horses' power required.***

Such is the simple process of experiment and calculation, by which the power of any machine may be accurately determined with M. de Prony's ingenious brake dynamometer.

The simplicity and cheapness of Prony's brake, the accuracy of its results, and the ease with which it may be applied to test the power of a motor, recommend it in the strongest manner to the attention of all scientific and practical men, who are interested in affairs of this nature. Tredgold recognized the simplicity and beauty of this meter of power, by recommending a simple Prony's brake, with one cushion of wood attached to the lever, and a friction strap of wrought iron, tightened by a single screw, to be applied to the fly-wheel shaft of steam engines, as the cheapest and best means of measuring the power developed by them; (see Tredgold on the Steam Engine, edition of 1838, p. 270,) and to this purpose the brake has been successfully applied, both in Germany and France.

Already we find, in the late French treatises upon mechanics, long and valuable tables of the exact power required to drive a great number of the machines in common use; these interesting results have nearly all been obtained by means of Prony's brake, and a little well-directed exertion on the part of our numerous practicians, would soon enable us to form tables of the power needed, to impel most of the machines used in the arts amongst us.

It is scarcely necessary to say that such tables, whilst they would be invaluable to constructors of works, would be deeply interesting to all mechanical philosophers, and might be the means of preventing a great waste of money, which is continually occurring, from the erection of motors utterly disproportioned in power to the machines they are intended to actuate; examples of which will doubtless occur to most of our readers, as they exist in every quarter of the country.

M. Morin, in the experiments which he made upon *turbines*, (subsequent to those on other water wheels,) used a brake very similar to that lately employed by the writer; and, in his account of these experiments, published at Metz and Paris in 1838, under the title of "*Experiences sur les Roues Hydrauliques a axe vertical appellées Turbines*," he describes this brake, and the method of using it, substantially, as follows:

*Of the Brake applied by M. Morin to the Turbine of Mullbach.*

"This brake was formed by a pulley of (1.25 m.) $4\frac{1}{10}$ feet in diameter, and of about (0.25 m.) $\frac{82}{100}$ of a foot broad at the throat, which had been turned with care, as well as its edges, and was well centred and wedged upon the upper part of the shaft of the turbine, which had not yet received the spur gearing which belonged there."

"The two jaws of this brake were of wood; the length of the lever, measured perpendicularly to the cord to which the charge was suspended, was found to be (2.99 m.) $9\frac{81}{100}$ feet."

"A cord fixed to the wood-work at (6 or 7 m.) $19\frac{68}{100}$ to $22\frac{97}{100}$ feet high, sustained the end of the lever, and a plumb line indicated the

position which it ought to preserve, while its acting length was perpendicular to the direction of the cord, which, passing over a fixed pulley, sustained the load."

*Precautions taken by M. Morin to insure regularity of movement with the above Brake.*

"To maintain the surfaces in the same state of humidity, we introduced, near the wheel, the fire engine of the establishment; and a watering pot was suspended above the cushion of the brake, in which a notch was made, whence the water poured on it."

"The men, in working the pump, directed a regular current upon the rubbing surfaces, which were thus perpetually cooled and lubricated to the same degree."

"We obtained, in this manner, such regularity in the action of the brake, that, when under the same charge, it has sometimes moved more than one half hour without suffering the least oscillation, and without any necessity of acting upon the screws."

"In others of our experiments, the oscillations of the lever below the vertical of the plumb line had not exceeded (0.02 to 0.03 m.) $\frac{6}{100}$ to $\frac{8}{100}$ of a foot, either way; and the stay pieces, disposed as a precaution, served only for the moments of interruption."

"We have not used a kilogramme of grease in making all these experiments, and although I already well remembered employing this dynamometrical apparatus with success, I had never seen it move with such perfect regularity."

"Thus aided by these easy and cheap means, I consider as entirely useless superfluities, all the modifications proposed or adopted by divers engineers in the simple arrangement originally proposed by M. de Prony."

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Where the shaft upon which the brake is mounted revolves at a very high velocity, some difficulty may be experienced in counting correctly the number of revolutions made in a definite time; and the writer will observe that, in experimenting on the turbine of Moussay, where the speed ran as high as 250 revolutions in a minute, M. Morin found it necessary to cause a spring blade to strike a projecting key at every turn, and then counted their number by *sound*. To count the number of revolutions, we have found it convenient to cut a nick in the shaft upon which the pulley is mounted; this shaft being smeared with oil, and the nick being rounded a little on all its edges, then, by holding a finger upon the shaft, so as to be touched by the nick at each revolution, the number of turns in a minute may be counted by the *touch*, with ease and accuracy.

Either *by touch*, *by sound*, or *by sight*, there are various modes, which will suggest themselves to the practical mechanic, by which all difficulty in accurately determining the number of revolutions in a given time, may be overcome.

We will now make a few observations relative to the strength proper to be given to *the cast-iron pulley of a brake*, and to the best mode of mounting it, where the shaft of the motor experimented upon

is of *cast iron*, and admits of its being placed upon the intended seat of a spur, or bevel, wheel.

*The brake pulley ought not to be less than two feet in diameter, and should be of the same strength and dimensions as would be required by a cast-iron spur wheel, to transmit the maximum power of the motor, at the minimum speed.*

It should, in fact, be a spur wheel, with the teeth removed, the face turned smooth, and having, in addition, either one, or two, projecting flanches, according as it is designed for application either to vertical, or horizontal axes.

The dimensions of a *brake pulley*, upon the above supposition, may very easily be calculated by the well known rules in use among machinists for determining the proportions of spur and bevel gear.

It will be observed that the pulley of the brake used by the writer was much lighter than a spur wheel would have been, to transmit the greatest strain it was employed to measure, or $14\frac{4}{10}$ H P, at sixty revolutions per minute; *it was, in fact, too weak*, and, judging from the symptoms of weakness displayed by it in an early stage of the experiments, it would undoubtedly have broken, had not the precaution been taken to fill the vacancies between the arms with oak plank, neatly scribed to fit them.

The mode of mounting the *brake pulley* upon the shaft of the motor under trial has usually been, as is described by M. Morin; the eye of the pulley being considerably larger than the shaft, (to enable it to fit various arbors,) and wedges being well driven to fill the vacancy; while, with the view of enabling the brake to be easily applied upon any shaft without displacing it, the pulley has usually been made in two parts, and joined with screws, whilst embracing the shaft.

Though this arrangement undoubtedly possesses some advantages, still the writer has found the method of fixing the pulley upon a cast-iron shaft, by wedging, to be so unsatisfactory, as to induce him to think that it is decidedly better, whenever practicable, to bore out the eye of the pulley so as to fit the seat of a spur, or bevel, wheel, and key it fast there with a single key, in the approved manner now used for staking on gearing; and although this mode will require a separate brake pulley for each size of shaft experimented on, still, its steadiness, firmness, strength, simplicity, and the certainty that the pulley will be accurately adjusted in both directions by the quick and simple operation of driving the key, *all combine to recommend this plan in practice.*

Before applying, to the turbine at the Rockland Mills, the brake pulley we have above described, the writer tried one that was made in two equal parts, screwed together at *f* and *e*, (see fig. 3) and of which the eye was about two inches larger in diameter than the shaft; it had *four* centering screws, at the points marked *a*, *b*, *c*, and *d*, and, after being centred, it was wedged fast to the shaft, with iron and wooden keys.

Several difficulties occurred in using this brake pulley:

1. With only *four* centering screws, placed singly, though they enabled it to be easily centred horizontally, it was found impossible to

bring the brake pulley truly into a horizontal plane, so as to run without wabbling.

2. The moment an experiment was begun, the centering screws rapidly unscrewed themselves by the jar of the machine.

3. The iron wedges, between the eye and the cast-iron shaft, would not hold, and wooden wedges, being tried, failed also.

4. Being nearly of the same dimensions as the other brake pulley, it was too weak.

Though the first, second, and fourth objections admitted of being remedied, by using *eight* screws, placed vertically in pairs, instead of *four*, by using lock nuts, and by augmenting the strength of the pulley, still, as it seemed very doubtful whether or not the third could be so easily overcome, the writer resolved to adopt the other mode of mounting the brake pulley, which, as we have before said, *proved entirely satisfactory.*

As a proof that brake pulleys, of the size of those above referred to, are too weak to sustain a power of $14\frac{4}{10}$ horses, at sixty revolutions per minute, the writer will mention, that, in applying to the last mentioned pulley, (which was mounted with wedges,) a power of about thirteen horses, at a speed of near sixty revolutions, it burst with a loud crash into several pieces, and brought that series of experiments to an abrupt termination.

We will now conclude by again recommending the brake of M. de Prony to the attention of practical men, *as the cheapest, simplest, and most convenient dynamometer that has yet been invented, to measure the power of machines.*

**Philadelphia, March 1, 1843.**

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## *Mr. Vignoles' Lectures on Civil Engineering, at the London University College.*

[Continued from Page 163.]

### SECOND COURSE. LECTURE IV.—LAYING OUT RAILWAYS.

In the preceding lectures, the subject of the motive power had been much enlarged upon, from its necessarily influencing the manner of laying out a line. Mr. Vignoles said the student may be referred to study, at greater leisure and in detail, the principles laid down in the works of various authors, on laying out both roads and railroads—M'Neill, Parnell, Navier, Tredgold, &c.,—and the rules laid down by them may be taken as sound first principles, though modified at present by the improvement of motive power, and other causes, which could not have been known *a priori.* Railroads have so completely superseded many of the principal roads, and the public convenience has been thereby so much interfered with, that it becomes a matter of importance to run the trains as often as possible, and this becomes a new element in laying out a line of railway. Hitherto this has been done under the impression that the engines would always carry maximum loads, and though it is true that main lines radiating from the metropolis, into which a number of tributaries fall, may be laid out with a view to maximum loads, yet it becomes a consideration

whether it would not be better, in general, to lay out railways with a view to the trains going often, and with light loads, and thereby to make the gradients suitable to the ground over which they pass. On this subject, Mr. Tredgold has always judged soundly. Seventeen or eighteen years since, he made various calculations on the comparative expense of ascending and descending inclined planes, and of cutting them down to a level; and he states, in his Treatise on Railroads, that it will be much less expensive to follow nearly the undulations of the surface, and "if a few examples (of the comparative expense) be added, it will assist in removing those extravagant notions of cutting and embankments, by which the capital of the country is wasted in unprofitable speculations." But the practice of engineers has been directly opposed to this, although we had almost a daily improvement in locomotive power, affording means of overcoming the difficulties of steep gradients. Before determining upon the inclinations which he will adopt, therefore, the engineer should make estimates of the comparative expense of forming and working flat gradients, and gradients of an inferior description, and it will be found that gradients of fifty, sixty, and even eighty, feet in a mile, may be advantageously introduced, especially where the traffic is not very considerable. And if lines were laid out upon these principles, instead of the traveler being overcharged with the expense of the capital sunk, as at present, he would be charged with the expense of the motive power, which bears a very small proportion to the total amount exacted from passengers. Locomotive power only is scarcely more than ¼d. per passenger per mile, whereas the ordinary charge to passengers is 2d.; and this may explain why railway companies do not lease the working of their lines, for they make most of their profit as carriers, and not as capitalists.

In laying out railways, there are generally two distinctive descriptions of country which the engineer meets with, each of which requires a different description of treatment with respect to his operations. The first is where there is a certain summit, or ridge of country, to be surmounted; the rule in this case will apply both to roads and railroads, viz., to get a uniform inclination, if possible, up to the summit; but, if that be not practicable, to lay out the line in stages, taking care that, having once attained any intermediate elevation, the line does not, if possible, descend again. In a country of this description, there will be much more difficulty in the details than in striking out the first general idea, for it will require the greatest care and patience to lay out the line so as to ascend to the summit at the least possible expense, by winding along the sides of hills, and crossing lateral valleys and ravines to the greatest advantage, &c.

The other description of country is where the extreme points of the line to be laid out are on a level, or nearly so, and the ground varies. In this case his judgment will be principally exercised in determining the general direction of the road, in taking trial levels to determine the line of least cutting and embankment, in avoiding valuable property, and in securing the largest amount of traffic; and in a country like England, which is so full of improvements, gentlemen's seats,

roads, streams, &c., it is an exceedingly complicated duty to make choice of the best line under such circumstances; but it may be laid down as a general rule, that, in any difficulty, it is always better to incur a positive known expense, which will not entail future liability, than, by diminishing the expense in the first instance, to run the risk of undergoing future loss. Thus, for example, if a line of railway upon a slight embankment should cross a road on the surface of a wet or marshy country, it will be better to raise the road to a sufficient elevation to pass it over the railway, though the height of the bridge and approaches be thereby greatly increased, than, by slightly lowering and passing it under the railway at a greatly reduced expense, to render it liable to be continually laid under water. And these are the kind of circumstances that require so much care and consideration on the part of the engineer, to enable him to judge of the comparative amount of cost and maintenance of the different systems which he can adopt, and to regulate his designs accordingly. Now we might go on thus increasing railway gradients until they approached nearer and nearer to those of a turnpike road, were it not for the difficulty of regulating the descent of them with safety. On a turnpike road, writers have suggested that from 1 in 36 to 1 in 40 is the best slope, because horses may gallop down without danger, and, at the same time, it is a good trotting road upwards. But on railways it is not safe to go down such inclinations as that. Professor Barlow lays down that when the inclination is greater than 1 in 160, all advantage from gravity in the descent is lost, from the necessity of applying the brake, and he has formed tables to show the amount of loss sustained in the ascent; thus, he states that going up one mile of 1 in 100 is equivalent (of course with a maximum load) to going 2½ miles upon a level; but he will not allow that any corresponding advantage is gained in the descent of this, or any plane steeper than about 1 in 180. Now, if this be the case, we must have a totally different set of elements in forming lines of railway from what I have been laying down. But, as has been already stated, this is not the case in practice, for trains can have, with perfect safety, the full benefit of gravity on all descents up to 1 in 100, and the engines seldom carry maximum loads. The same line of argument has been pursued with respect to turnpike roads, where, however, there are many circumstances in operation which do not occur on railways—such as the unsteadiness of horses and coachmen—which influence the question; but the great point to be considered is, whether it is most economical to lay out railways with respect to stationary, or to locomotive, power. On this subject M. Navier very sensibly remarks, that, great rapidity being the characteristic of railways, it has been considered necessary to employ locomotive engines, which system presents an important advantage in being able to increase gradually the number of engines, as the demands of commerce require it; whereas, on the stationary system, it is necessary to provide at once for the greatest amount of traffic that can ever occur. But, in the event of this increase, we have still the means of using light and frequent trains for transporting a heavy traffic over a line of inferior gradients, and reducing the charge of the

interest of that capital to the public. But, whatever be the description of country which the engineer may meet with, he should, first of all, make, or procure, detailed plans on the largest scale, and upon them lay down a number of surface levels, and from them, as from a model, to find the line of least expense and greatest accommodation. The magnificent Ordnance Maps of Ireland, from their great scale and numerous surface levels, will render the task of the engineer, in that respect, easy, should the long deferred introduction of railways into that country be ever carried.

(To be continued.)

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## *Facts and Observations on Four and Six Wheel Engines.*

*By* JOHN HERAPATH, ESQ.

[Continued from Page 89.]

*London and Brighton Railway.*—After the long journies I had in the winter, I was in hopes I had terminated my labors. It being, however, the wish of the managers of the *Railway Magazine* that I should go over the Brighton line, I asked and obtained permission of the Company to do so.

Six projects for lines were once candidates for public favor to Brighton—Stephenson's, Cundy's, Gibbs', Palmer's (or the South Eastern), Vignoles', and the one which is now made, Sir John Rennie's. For two successive sessions the parliamentary warfare was carried on. At one time it was calculated that the united parliamentary expenses amounted to upwards of £1,000 a day; and I have heard it said, but with how much truth I do not know, by one of the parties deeply interested, that, including all the expenses and all the prior costs of all the Companies, there was near £300,000 spent on the Brighton lines before the present Act was obtained. I have, however, also heard from others, apparently equally entitled to credit, that not above £160,000 was spent.

Perhaps, in no parliamentary contest was there ever more acrimonious feelings displayed than in this one. Engineers, counsel, solicitors, parliamentary agents, and a legion of witnesses reaped a rich harvest out of the pockets of the subscribers; such a time will hardly ever come for them again.

At length Captain Alderson was appointed by the Government to report upon the best line to Brighton, and decided in favor of this one, as surveyed and proposed by Sir John Rennie. The Act was then speedily obtained, and received the royal assent July 15, 1837. Soon afterwards Mr. Rastrick was appointed the acting engineer, and July 12, 1841, 28 miles of the line, namely, to Hayward's Heath, were opened, and on the succeeding 20th of September, the remainder. Thus, in the short space of about four years, was the land bargained for and conveyed to the Company, and the works, which were said by one of our most experienced engineers never could be executed, finished, and the line in operation for the accommodation of the public and the benefit of the Shareholders.

Such expedition is highly creditable to the Directors, engineer, and not less to the Proprietors, by whose ready and efficient supplies they were enabled to surmount the difficulties, and complete the undertaking. But, probably, the best commentary on the conduct of all, is the following comparison with the other metropolitan railways:—

| Names. | Passing of Act. | Opened entirely | Time. | Length |
|---|---|---|---|---|
| | | | ys. mo. | miles |
| South Western, | July 25, 1834. | May 11, 1840. | 5 10½ | 76½ |
| London & Birmingham, | May 6, 1833. | Sep. 17, 1839. | 6 4½ | 112½ |
| Great Western, | Aug. 31, 1835. | June 30, 1841. | 5 10 | 118½ |
| Croydon, | June 12, 1835. | June 1, 1839. | 3 11½ | 8½ |
| Eastern Counties, | July 4, 1836. | Not yet. | | 126 |
| South Eastern, | June 21, 1836. | Not yet. | | 65 |
| North and East, | July 4, 1836. | Not yet to Cambridge. | 5 7 | 53 |
| London and Greenwich, | May 17, 1833. | Dec. 24, 1838. | | 3¾ |
| Blackwall, | July 28, 1836. | Aug. 2, 1841. | 5 0 | 3½ |
| London and Brighton, | July 15, 1837. | Sep. 20, 1841. | 4 3 | 41¾ |

A more creditable proof of industry could scarcely be expected in a work of such acknowledged difficulty. The line nearest to it in difficulty, as a whole, is the London and Birmingham, and that, with the best and most prompt and wealthy proprietary in England took above two years longer, or nearly half as long again.

Before the Act for this railway was obtained, parties for the sake of getting supporters to their various schemes to Brighton, talked of capitals of £800,000 or £900,000, but as soon as it was certain that this line would be carried, common sense dictated to them a more reasonable sum. Accordingly the capital was fixed at £1,800,000, in 36,000 shares of £50 each. By the statements in the R. M. vol. iv, p. 470, it appears that the capital, after deducting £5 per share undertaken not to be called up, is £1,620,000, and that the cost as per last report was £2,568,212 or 1.59 times the estimate. Comparing this with the other finished metropolitan lines, and we have—

| Names. | Estimate. | Cost. | Excess per ct. |
|---|---|---|---|
| | £ | £ | |
| Birmingham, | 2,500,000 | 5,894,551 | 135 |
| Great Western, | 2,500,000 | 6,341,326 | 153 |
| South Western, | 1,000,000 | about 2,000,000 | 100 |
| Croydon, | 140,000 | 637,875 | 355 |
| Greenwich, | 400,000 | 934,774 | 133 |
| Blackwall, | 600,000 | 1,071,717 | 79 |
| Brighton, | 1,620,000 | 2,568,212 | 59 |

Thus it appears that the line was finished in a less time than either of the metropolitan lines, the Croydon only excepted, and at a far less proportional excess of expenditure. It is usual for the excess to be in proportion to the rapidity of execution. As, for instance, the Croydon was executed in less time than any line, and cost 355 per cent. above its estimate. But the Brighton line finished within 3½ months of the same time, cost only 59 per cent. more than its estimate, though pre-declared impossible.

I presume the two facts of time and excess of expenditure of this line, which was so ridiculed for its supposed impossibility, is the best answer that can be given to the calumnies which have been heaped upon the administrative and executive departments. The only railway which has approached it in rapidity of execution, has exceeded its estimate nearly six times as much ! !

*Description.*—This line commences at the Greenwich station at London Bridge, which line it takes for 1¼ miles, then the Croydon for 7 more, and passing Reigate and Cuckfield each about 1½ miles to the east, takes an almost direct southerly course to Brighton, which it reaches in 50 miles and 50 chains. In its length there are three summit levels, namely, one near the middle of the Merstham tunnel, one a little before entering the Balcombe tunnel on the London side, and a third just at the Brighton end of the Clayton tunnel. From London to New Cross, 3 miles, it is nearly level; for 2 miles 50 chains after there is a rise of 52.8 feet a mile; thence to the Croydon Junction, 8¾ miles from London, it is again nearly level; after that, for 8 miles to the middle of the Merstham tunnel there is a continued rise of 20 feet a mile; then another uniform descent for 7 miles of 20 feet a mile to the Horley station, about 25 miles from London; then an ascent for 2¼ miles of 11½ feet per mile, followed by one of 20 feet a mile for 4 miles; after this come 5½ miles of descent of 20 feet a mile, 3 miles of 16¾ feet a mile, which brings us to 40 miles from town. Then follows a rise for a mile of 20 feet, for a 1½ mile another of 16½ feet a mile, and then 3 miles of 20 feet a mile. Hence there is a uniform descent for 4¾ miles to Brighton of 20 feet a mile.

The Brighton line therefore consists of six long inclines, the prevailing gradient of which is 20 feet a mile.

The Shoreham branch is a continued descent from Brighton to very near Shoreham.

This, and the South Eastern Dover line, run together until they come to within a quarter or half a mile of the Red-hill station, that is for 20¾ miles. The part, 12 miles, between the Croydon Junction and Red-hill, will be a sort of common ground for the two Companies, as soon as one half the cost of construction, about £350,000, including interest of capital and other expenses, shall have been paid over by the South Eastern to the Brighton Company. Each party is to keep up its own half of the road, and to run toll free over the other. The part belonging to the Brighton is the first portion after passing the Croydon Junction.

At first sight it would appear that the Brighton Company had the worst of the agreement, as their portion of the ground is the least expensive and the most costly to keep up, being chiefly embankment; but this is not so, as the embankments are chalk. All the risk of finding the money and the anxiety and trouble of construction have likewise been the share of the Brighton Company. But then they have had the advantage of getting the line sooner opened, and the twelve month's profit arising from the use of it; for, had any portion of these twelve miles been incomplete, it would have been nearly tan-

tamount to having the whole line unfinished. On the other hand, there is no doubt but that the Brighton Company having finished their line, has been a great spur to the South Eastern Shareholders, who at one time appeared very lukewarm towards their undertaking.

Upon the whole, the advantages are pretty equally balanced between the two Companies, and it is quite obvious that the agreement itself has been beneficial to both. It is therefore to be devoutly hoped that its consummation will be effected in harmony and good fellowship between them. Every one who attempts to prevent it, by word or deed, is an enemy to both, and as such ought to be regarded. Questions arising out of the cost of the joint portion are not here contemplated. From the wording of the act it is presumed there can be no material points for dispute, and it is to be hoped the good sense of both parties will not allow minor matters to form cause of dissension.

Could the Companies have made a common station at Red-hill, instead of having two near half a mile a part, it would have been much better for them. As it is, however, the next best thing they can do is to work the whole distance to Red-hill in joint trains. This would save all danger of collision, all perilous rivalry between the men, and, what is of material consequence to the Shareholders of both Companies, nearly one half the expense.

Exclusive of considerations of traffic, such a plan is of more consequence to the South Eastern than to the Brighton Company, inasmuch as the engineer of the former Company having laid his rails at a 4 feet 9 inch gauge, instead of a 4 feet 8½ inch, if the engines are made to a 4 feet 9 inch—which, as being the longer length to run over, it is probable he has—they will travel with more friction and expense on the Brighton rails than they will upon the South Eastern. It will therefore be preferable for him to keep his engines to work on his own line, to which they are better adapted.

*Character of the Works.*—The works are generally heavy, but are well executed. The Ouse Viaduct, for instance, is a noble and handsome structure, entirely of brick, except the parapet. It is 1,434 feet long, and 96 from the top of the rails to the river. It consists of 37 semi-circular arches, each 30 feet span, built on lofty piers. The parapet is of light Caen stone, and is said to have cost no more than Portland stone. At each end of the viaduct are 4 tasty temples or sentries, which add much to its beauty. The arches are 1½ brick thick, and the engineer has made a considerable saving in the brickwork by employing arches of 30 feet span instead of 60 feet. The beauty of this viaduct, the finest work on the line, and second perhaps to none in the kingdom, is lost to the traveler, and can only be seen in the valley. I have heard that an hotel, gardens, &c., are to be laid out in the neighborhood.

I had an opportunity of examining this structure in company with several professional gentlemen, who one and all much admired its design and execution. With the closest attention not the slightest distortion was detected.

Having its dimensions, I therefore made an approximate calculation of what ought to be its cost according to other similar constructions I

had seen, which came out from £70,000 to 80,000, but I heard then that £62,000 was much nearer the mark, upon which one of the engineers, Mr. Blackmore, observed to me, "then it has been built a great deal too cheaply."

Odium has been attempted to be thrown on the engineer for not using in the Red-hill and other embankments the chalk excavated on the other parts of the line. If he could have commanded this chalk he would have been mad not to use it. But the fact is, when it was wanted it could not be had, for the best of all reasons, that it was in the unexecuted tunnels, and on the wrong side of them; and had he made the embankments wait for this chalk, the line could not have been finished under 10 years instead of 4¼.

The stations, 11 in number, are neat and simple buildings, upon which it is quite evident not too much money has been laid out. At Brighton, however, the station is large and capacious, and contains an excellent suite of rooms, well arranged for the administrative and executive departments. The carriage shed is divided into two compartments, one for the *London* and the other for *Shoreham* traffic. This shed is one of the most beautiful, light, and airy structures of the kind I ever saw. It is 295 feet long and 166 feet wide, and built upon a gentle curve, which gives to its trussed iron roof a very pleasing appearance. I am told the roof of this shed cost about £5,000. The shed is completely open at the sides, the offices standing across the end, and the roof is supported on numerous iron pillars. The curved girders supporting the trussing of the roof are not cast in one piece from column to column as is usually done, but the spandril forms the centre of the girder, whose ends abut in the middle of the arch. By this means there is no lateral thrust upon the iron columns; the pressure is vertical, and the stress on the girder thrown where space affords the best opportunity for giving it strength, namely, at the spandrils.

The trussing of this roof, though it appears complex, is simple, light, and ingeniously contrived. Almost all the forces are tensile, and the roofing, therefore, composed chiefly of braces. Where there are struts, they are chiefly tubular, and of course light and strong.

At each station there is an additional pair of side rails on each side. As soon as a train enters this siding, the switch is closed, and no runaway engine or careless fellower can, by any possibility, run into the standing train, but must shoot ahead. Danger, then, from such an unfortunate occurrence as happened at the junction of the North Midland and York and North Midland, is not avoided, but absolutely prevented. This is a plan of the engineer.

There are five tunnels reckoning from London, the Merstham, 1,830 yards long, and 17 miles from London; the Balcombe, 1,139½ yards long, and 32 miles from town; the Hayward's Heath, 248½ yard long, and 38 miles; the Clayton, 2,256¼ yards long, and 44 miles; and the Patcham, 491⅔ yards long, and 47½ miles from London. Of these tunnels the Merstham has a summit level about the middle of it, and dips, therefore, both ways; the Balcombe commences near the next summit and dips towards Brighton; the Hayward's Heath dips

in the same manner, towards Brighton; the Clayton terminates near the third summit level, and dips towards London; and the Patcham dips towards Brighton.

I had an opportunity of walking through the Balcombe tunnel with Mr. Rastrick, the engineer-in-chief, who was anxious to afford me the best opportunity he could to inspect it; Mr. Blackmore, engineer of the Newcastle and Carlisle, and several other gentlemen. We were all struck with the difficulties the engineer must have had to encounter with the water in some of them. Before he could proceed with his work he was obliged to run drift ways of from a quarter to half a mile, and build culverts, equal to our London sewers, to carry off the water. In some of the shafts garland gutters are run round them, for the purpose of catching the water and conducting it to pipes leading to the main culvert beneath. To prevent the wet also from falling upon the rails and trains, lead sheeting across some parts of the roofs of two or three of the tunnels is put, which conveys the water down pipes by the sides into the drains.

These lead sheetings have occasioned curious mistakes to be made by some persons, who have run away with the notion that they were used as props to support the arches. Poor props, indeed, lead would be for tunnels.

In riding through these tunnels upon the engine, I was induced to believe that they were more than usually dry, and it was not until I had an opportunity of walking through one of them, that I could form any idea of the trouble that had been taken to render them so.

The bricks with which they are built are all of the best and hardest kind; the arches are laid in cement.

The curves of these arches are ovals, with the longer axis upwards and the shorter across the tunnel, at some feet above the level of the rails, the segment apparently cut off by the ground forming the invert; for I understand they have all inverts. These figures have a pleasing effect. About every 200 yards, in each side of the tunnel we walked through, are spacious recesses, of some 15 feet deep by perhaps 6 wide and 8 high, called sanctuaries, for the purpose of retreat for the workmen from the trains, should two of them meet in a tunnel, and also as a provision for the safety of passengers in case of accident or detention in the tunnel. This, though it may never be wanted for passengers, is a very prudent precaution.

I noticed that in every case the engineer has made his tunnels straight, and put his curves before and after them. By this expedient any obstacle is immediately seen on entering a tunnel, and of course guarded against. All the long tunnels are lighted with gas or oil lamps, which, though but of little benefit to the passengers in their transit, would be of considerable utility in case of accident. They are also thought to be serviceable to the engine drivers by illuminatting the rails before them for very long distances, but I had no means of proving this.

All the entrances to the tunnels are very simple and neat. That, however, on the London side of the Clayton tunnel is a beautiful exception. It consists of a very pretty facing of brickwork and stone,

a castellated parapet, and a neat octagonal tower on each side of the tunnel entrance, with apartments within them for the policeman. Placed in the midst of a deep cutting, these towers, with their old fashioned loop holes, present a formidable and menacing appearance to intruders.

During my several trips on the engine right through the line, and two special trips over portions of it, I paid particular attention to the cuttings. They are in certain places steeper than I have seen them upon almost any other line. But the material the engineeer had to cut through seems to justify him in the course he has adopted. For example, the cutting in the Shoreham branch, near Brighton, is through a chalk of so shaky a character that one might have been afraid to let it stand at less than ¾ to 1, yet it has now stood for two years at nearly as steep an inclination as the best on the line. The principle apparently followed was to make the slopes in the same cutting according to the material he had to deal with. In the deep cutting on this, the London, side of Merstham tunnel, for a considerable height the chalk is hard and solid, and the slope cannot, I should imagine, be more than 1 to 6, but higher up, where its character changes, it is rounded off much more obliquely. Had he not studied the character of the soil, and had he made the whole cutting of one uniform slope, such as the upper strata required, I hesitate not to say that this cutting could not have been made for at least double the quantity of material and double the expense.

It is by this close adaptation of his works to circumstances, and effecting savings where he could, which have enabled him to be less sparing in difficulties on other parts of the line, and yet keep the whole cost so much nearer the estimate than others have done. With some of his tunnels he must evidently have had a good deal to contend with, and his patience and skill must have been not a little tried.

The rails (75 lb. rails) are laid chiefly on cross sleepers three feet apart, but in one or two places I observed stone blocks, and on the Ouse viaduct longitudinal bearings. The greater harshness of these bearings in changing from them to cross-sleepers, or *vice versa*, though they have two feet of ballast and mould under them, was very apparent.

*Trains and Staff.*—From end to end the trains work very well and keep their time very correctly, but in some of the intermediate stations they are not so accurate. I have known them five and near ten minutes out, and yet they have arrived at the terminus, without any sensible hurrying, to a minute. A remedy might easily be made by a little alteration in the time-bills. It appears to me that there has been some misapprehension in framing these time-bills. Thus, from London to Croydon, all heavily up hill, 23 minutes are allowed, while from Croydon to London there are 30 minutes. Again, from Hayward's Heath to Brighton, the time is 30 minutes, but from Brighton to Hayward's Heath 24 minutes. The same happens between other places. I have no doubt this discrepancy is not the effect of accident, but I cannot see the utility of it; and the practical advantage, as far as my observations have gone, has not presented itself to me.

Their engine drivers are, from all that I have seen, a sober and

steady set of men, and such as well understand their business. The entire staff of the Company is indeed a very good one, exceedingly attentive to their duties, and under good discipline. It is not by their attention on the regular trains that I judge of them, but I have had other opportunities of observation, and I invariably found them at their post. Though I was a fortnight backwards and forwards on the line, I am not aware that I met with one churlish, inattentive policeman, conductor, engine-driver, or porter. In fact the Directors are generally so much about the works, and watching the operations so closely, that it would be a great risk for a man to be uncivil, or for a moment off his guard.

*Traffic.*—With respect to the traffic, to use an old hacknied phrase, "it is yet but in its infancy." If this line follows the example of the London and Birmingham, the Grand Junction, and Liverpool and Manchester, it will go on rapidly increasing for the next two or three years, and then will become nearly stationary, or, if not, its increase will be gradual and uniform.

(To be continued.)

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## Physical Science.

### *The late Eclipse..*

At the meeting of the Academy of Sciences, on the 22d ult., M. Arago made his promised communication, which has been so anxiously expected, relative to the eclipse of July 8th, as observed by him and other astronomers at Perpignan. We take our report from *Galignani:*—

"M. Arago began by stating that the object of himself and the gentlemen associated with him in the observations at Perpignan, was not so much to verify the accuracy of the calculations as to the precise moment at which the eclipse was to occur, as to determine as far as possible some undecided opinions as to the nature and character of the heavenly bodies on which our earth depends for light and heat; but, being provided with the means of ascertaining the exact moment of the eclipse, they did not, of course, neglect to record it. M. Arago expressed his surprise at having seen it stated by some observers that the phenomenon occurred precisely at the time predicted; for, according to his observation, it did not take place until from 30 to 40 seconds later than the prediction. This error of calculation, he observed, might appear to many to be too trifling to deserve notice, but, in his opinion, it was inconsistent with the progress made in astronomy, and it would be necessary for the honor of the science to trace its cause and prevent its repetition. The learned academician then proceeded to communicate the result of his observations on the halo which appears to surround the moon after the entire disappearance of the sun, and which modifies the darkness occasioned by the eclipse. Plutarch says—'The moon, in an eclipse of the sun, allows a portion of the sun's light to extend beyond her own edges, and thus total darkness is prevented.' The lunar halo is more particu-

larly described by Plantade and Clapiés in their observations of the eclipse of 1706. 'As soon as the sun was wholly eclipsed,' say they, 'the moon appeared to be surrounded by a very white light, forming round the disk of that planet a halo three minutes in width; within this limit the light was the same throughout, gradually failing, and at length dissipating itself in darkness.' The width of this luminous appearance, however, varies according to the eclipse. In 1719 Halley found an extent of two minutes and seven seconds; in 1806 the observation of an astronomer in America gave six minutes. At Perpignan, on the 8th of last month, the width was three minutes and thirty seconds, and it did not vary during the different phases of the eclipse. M. Arago had recommended to his colleagues to make it an important point to ascertain whether the halo had its centre on the sun or on the moon, the existing opinions on this question being of a conflicting nature. Halley and Lonville have affirmed that the centre of the halo coincided with that of the moon; whereas, according to Maraldi and Ferrer, the centre is always that of the sun. The astronomers of Perpignan report that the opinion of Halley and Lonville is the correct one. They measured the luminous coronet with the greatest care, and found it equal on both sides, which led them to conclude that the white aureola, which extends beyond the obscured body of the moon, is not produced by the sun's atmosphere, and is simply a phenomenon of luminous diffraction. The serpentine lights observed on the surface of the moon in 1715, by Halley and Lonville, and which the latter regarded as lightning arising from storms in the atmosphere of the moon at the moment of the eclipse, were not seen at Perpignan. Some meteors, or shooting stars, were, however, observed. It is not improbable, therefore, that the serpentine lights noticed by Halley and Lonville were meteoric appearances brought by chance over the perspective of the superposed bodies. The Toulouse astronomers, in their account of the eclipse of July 8, state that they had observed a luminous opening in the edge of the moon, about forty seconds before the end of the eclipse, and they assign to it an extent of 156 leagues. A similar observation was made, by Admiral Ulloa, in 1778. The luminous point which he perceived on the north-west portion of the moon was, according to him, 109 leagues in length, being a narrow opening or perforation of our satellite, admitting a small portion of the sun's light. M. Arago, without absolutely denying the existence of this opening, states that, in the observations at Perpignan, there was nothing to confirm it. During an eclipse, the moon is designed in black, upon the sun, in its true form. The region of the sun which remains visible is, therefore, limited by two portions of circumference. In the points in which they meet, these two arcs, one dark, the other luminous, form two curvilinear lines which are called *horns*, and which are sometimes very thin and sharp. The luminous rays of the sun, which define clearly even the summit of the horns and surrounding parts, cross the surface of the moon to arrive at the earth. This preliminary description introduces some remarks by M. Arago, on the important question as to a lunar atmos-

phere. If, says this gentleman, the moon had a sensible atmosphere, these rays would deviate, the circular form of the sun would be affected, and the horns would show inflections and irregularities. Nothing of this kind was seen at Perpignan. It was only at rare intervals that the horns appeared mutilated, and they were never so completely. The observations on the bright spots of the sun led the astronomers of Perpignan to the same conclusion as to the non-existence of a lunar atmosphere. When the edge of the moon, during the eclipse, passed a solar spot nearest the black disk of the sun, it had the same luminous intensity as the remainder. This equality of light, says M. Arago, would not have existed if a vapor of any kind, even of no greater extent than the distance of the Luxembourg from the Observatory, had surrounded the moon as an atmosphere. The number of stars seen at Perpignan during the height of the eclipse was only ten, from which we may infer that the darkness was at no time great. The accounts given by the ancient astronomers of the eclipses observed by them are very different. According to them the darkness in some cases was more profound than that of night, and the stars shone with a brightness which filled the inhabitants of the earth with admiration and astonishment. It would appear, however, by the accounts of other astronomers who watched the eclipse of the 8th of July, that a greater number of stars was visible than that seen by M. Arago and his colleagues. This was particularly the case at Montpellier, and also at Milan, although without the central range of the eclipse. The thermometrical observations of M. Arago are less extensive than many persons could have wished. He is brief in his account of the change of temperature experienced during the maximum of the eclipse. He states, indeed, that the two minutes and a quarter of the total occultation of the sun sufficed to cool the atmosphere to such an extent, as to cause an abundant dew to fall upon the trees and plants, which were dripping with wet when the sun again made its appearance; but he has omitted to state with precision the degree to which the mercury fell in the thermometer. M. Lentheric, professor of mathematics at Montpellier, explicitly states, that at the commencement of the eclipse there, the thermometer stood at 18° centigrade (about 75 Fahrenheit). At the moment of its greatest obscurity, it marked only 15½°, but at the end of the eclipse, the mercury rose to 20. M. Lentheric relates a curious fact as to the termination of the phenomenon. The dazzling effect of the sudden return of the light, he says, was such, that he could not, at the moment, distinguish the hands of his chronometer, and therefore was unable to determine the moment with the precision desired. An interesting experiment was made by the Faculty of Sciences of Montpellier, to ascertain the luminous intensity at the different periods of the eclipse. The means employed was the daguerréotype. All the proofs gave a sufficiently defined image of the phenomenon to enable the members of the faculty to determine, by actual admeasurement, the relative apparent diameters of the sun and moon. At Toulouse, M. Flangerques not only noted down a fall in the temperature of 4° centigrade during the eclipse, but also saw the mercury fall in the

barometer. The mercury fell to thirty-one hundredths of millimetres below the height at which it would have stood if the difference of temperature had been the cause of the variation. This depression is, indeed, of itself unimportant, but it nevertheless shows a deviation from the normal action of the barometer, for it is known that this instrument usually goes on rising from the getting up of the sun until nine in the morning when it attains its maximum. M. Arago, in the course of his communication as to the observations at Perpignan, states, that during the latter period of the eclipse, he saw, on the edge of the black disk of the moon, a sort of protuberance of fire, two minutes in height, and presenting an appearance like that of the glaciers of the Alps illuminated by the setting sun. At Narbonne, the appearance was that of a distant light-house. M. Littrow, of Vienna, also noticed this protuberance, and gives to it an extent of five minutes, or the twelfth part of a degree. M. Bouvard, of Digne, distinguished luminous points proceeding from the edges of the moon, but he attributes them to divergent rays. There will naturally be much speculation as to the character of the protuberance noticed by M. Arago. Some members of the Academy have already thrown out the idea of a mountain of the sun rendered visible in the atmosphere of that body. The theory of Herschel is, that the sun, which is the source of light and heat to us, and which has been regarded as an incandescent body, is in reality dark and inhabitable. M. Arago, whilst he affirms that the protuberance which he observed is not of the moon—no such discovery having ever been made even with the most powerful telescopes—does not admit that it is a mountain of the sun, not that there is any thing repugnant in the laws of science in supposing the existence of a mountain of the sun, 17,000 leagues in height—or, according to M. Littrow's calculation of the extent of the protuberance, 50,000—for objects are only large or small comparatively, and Herschel has shown that the sun, by its prodigious mass, might have mountains, even 120,000 leagues in height; but M. Arago's doubts are founded on the diversity of opinions as to the character of this protuberance. This mountain, if it were one, would have presented a fixed projection and the same angle to each of the observers, which was not the case. M. Arago, therefore, is disposed to regard the phenomenon as one of diffraction. It is proposed, however, to determine this point by experiments with artificial means on the summit of some high mountain. Another curious circumstance mentioned by M. Arago, is the following:—At about the middle of the eclipse, M. Arago was able to perceive the whole disc of the moon. What was the light which enabled him to do this? It could not be the ash-colored light (*la lumiére cendrée*) left by the eclipse, for that is exceedingly feeble. There is, in this fact, a mystery which is perhaps impenetrable in the present state of astronomical science. The effect of the eclipse upon the population of Perpignan, who were watching it, is described by M. Arago as singular and even affecting. The gravest persons were unable to restrain expressions of joy when the sun re-appeared, and, whilst the eclipse lasted, anxiety was depicted on every countenance. At the foot of

the citadel in which the astronomers were making their observations, was a regiment of soldiers. They were laughing and full of gaiety until the face of the sun was obscured, when suddenly they seemed struck with dismay and stupor. The effect upon animals was so remarkable, that, if some portion of what is related did not rest on such good authority, it would not be credited. One of the friends of M. Arago had placed five healthy linnets in a cage. During the sudden darkness of the eclipse, three of the five died. The oxen formed into a circle, with their horns thrust forward, as if preparing for the attack of an enemy. At Montpellier, bats and owls left their retreats, and sheep laid down as for the night, and the horses in the fields were in a state of terror. In addition to these facts, it was stated to M. Arago, in the Academy, on the authority of M. Fraisse, a distinguished naturalist, that a swarm of ants in full march stopped short at the moment of occultation. Lond. Athenæum.

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### *Antarctic Expedition.*

The *Guernsey Star* has published the following extract, from a letter dated in May last, from the Falkland Islands:—"Captain Ross and the Antarctic expedition are now here. The *Erebus* and *Terror* came in contact, on endeavoring to escape an iceberg, in the seas of the Southern Pole. The expedition will positively be here for five or six months, to repair the vessels and to make observations. Captain Ross has erected an observatory at the old French fort built by Bougainville. A most interesting series of observations is carrying on. Those upon the pendulum are noted every quarter of an hour. Astronomical observations are also carefully made by the officers. Thermometers are placed both above the ground and under it; mine, with my barometers, are now doing duty with the rest, and have the honor to be registered also. The anemometers, showing the direction and force of the winds, will add much to the valuable information afford by Captain Sullivan, R.N., respecting these islands. Pluviameters are also carefully registered. A tide-gauge is by the jetty, and an excellent magnetic observatory, where the dip, intensity, and variation of the needle are carefully registered by able observers. The officers relieve each other in regular watches on these duties; and I never met with such devotees of science. You would be delighted to see Captain Ross's little hammock swinging close to his darling pendulum, and a large hole in the thin partition, that he may see it at any moment, and Captain Crozier's hammock is close alongside of it. The floor of this room is mother earth, from our want of timber. Captain Ross has been so kind, at my request, as to add to these observations another series, to ascertain the rate of evaporation in these islands; and Hooker, the botanist, is also so good as to draw up a report on the grasses, the prevailing gramina being considered as unknown in Europe. The splendid tussack grass is the gold and glory of these islands. It will, I hope, yet make the fortune of Orkney and Irish landholders of peat bogs. Every animal here feeds upon it with avidity, and fattens in a short time. It may be planted

and cut like the guinea grass of the West Indies. The blades are about six feet long, and from 200 to 300 shoots spring from one plant. I have proved, by several experiments, that one man can cut 100 bundles in a day, and that a horse will greedily devour five of these in the same time. Indeed, so fond of it are both horses and cows, that they will eat the dry tussack thatch from the roofs of the houses in preference to good grass. About four inches of the root eats like the mountain cabbage. It loves a rank, wet, peat bog, with the sea spray over it. Indeed, when the sea beats with the greatest violence, and the sea spray is carried farthest, then the tussack grass thrives best on the soil it loves. All the smaller islands here, though some of them are as large as Guernsey, are covered with tussack, which is nutritious all the year. The whole of the gentlemen in the expedition are delighted with the Falkland Islands, and express themselves as being more pleased with them than even with New Zealand. Some think them in every way better for colonization, even with the drawback of wanting timber trees. When the observations made during their voyage are published, you will be surprised at their favourable account of the climate. In addition to all these scientific observations, the surveying department is exploring and surveying different harbors, and sites for different objects in a new settlement."

Lond. Athenæum;

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## *Astronomical Clock.*

After four years labor, the repairs of the astronomical clock at Strasbourg are completed, and it will be set in motion on the meeting of the Scientific Congress on the 28th. In this curious piece of mechanism the revolution of the sun, the moon, and the planets are marked down with scientific exactness. Seven figures represent the seven days of the week, each appearing in its turn on the day allotted to it. The four ages come forward to strike the quarters, and the skeleton Death strikes the hours. At noon the twelve apostles advance in succession to bend down before the figure of our Saviour, who gives them the benediction. At the same moment a cock claps his wings and crows three times. It is said to be one of the most curious pieces of clock-work in Europe. Lond. Athenæum.

*Meteorological Journal for* 1842, *kept at the Lancaster Conservatory of Arts and Sciences, Lancaster, Pa., by* WASH. L. ATLEE, M. D., *Obs.*

| | Barometer. | | | External Thermometer. | | | | | Hygrom'r, 2P.M. | | | Sky. | | | | Winds. | | | | Clouds. | | | | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 7A.M. | 2P.M. | 9P.M. | 7A.M. | 2P.M. | 9P.M. | Reg'r | Mean | Dew point. | Diff. dw pt &tem | Wet bulb. | | 7A.M Days | 2P.M Days | 9P.M Days | | 7A.M Days | 2P.M Days | 9P.M Days | 7A.M Days | 2P.M Days | 9P.M Days | Rain Gauge. Inches |
| JANUARY. Att. ther. | 34.32 | 40.97 | 38.06 | | | | | | | | | | | | | N. | 1 | 0 | 4 | 0 | 0 | 0 | |
| | | | | | | | | | | | | | | | | N. W. | 6 | 8 | 6 | 5 | 6 | 1 | |
| | | | | | | | | | | | | | | | | N. E. | 3 | 2 | 4 | 0 | 0 | 0 | |
| Average | 29.37 | 29.40 | 29.45 | 30.06 | 40.22 | 33.39 | 27.73 | 33.78 | 30.87 | 10.77 | 34.48 | Ent. clr | 9 | 5 | 13 | S. | 1 | 1 | 3 | 0 | 0 | 0 | |
| Maximum | 30.02 | 29.94 | 29.96 | 43.00 | 58.50 | 52.00 | 39.00 | 47.25 | 53.90 | 20.60 | 56.00 | Ent.cld | 8 | 10 | 9 | S. W. | 11 | 12 | 6 | 2 | 4 | 0 | |
| Minimum | 28.95 | 28.93 | 28.99 | 12.00 | 24.00 | 17.00 | 12.00 | 20.50 | 3.40 | 0.00 | 20.00 | Par. clr | 14 | 15 | 9 | S. E. | 5 | 4 | 6 | 0 | 0 | 0 | |
| Range | 1.07 | 1.01 | .97 | 31.00 | 34.50 | 35.00 | 27.00 | 26.75 | 50.50 | 20.60 | 36.00 | | | | | W. | 4 | 3 | 2 | 7 | 8 | 2 | |
| | | | | | | | | | | | | | | | | E. | 0 | 0 | 0 | 0 | 0 | 0 | |
| Omitted | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 2 | 2 | 2 | | 0 | 1 | 0 | | 0 | 1 | 0 | 8 | 8 | 15 | 0.720 |
| FEBRUARY. Att. ther. | 37.00 | 43.87 | 39.87 | | | | | | | | | | | | | N. | 0 | 0 | 1 | 0 | 0 | 0 | |
| | | | | | | | | | | | | | | | | N. W. | 5 | 7 | 8 | 4 | 6 | 0 | |
| | | | | | | | | | | | | | | | | N. E. | 7 | 2 | 2 | 0 | 0 | 0 | |
| Average | 29.43 | 29.42 | 29.45 | 32.54 | 43.37 | 34.85 | 30.05 | 36.10 | 29.50 | 13.61 | 36.02 | Ent. clr | 9 | 4 | 17 | S. | 0 | 1 | 2 | 0 | 0 | 0 | |
| Maximum | 29.87 | 29.79 | 29.77 | 59.00 | 70.00 | 62.00 | 59.00 | 64.50 | 62.09 | 41.05 | 65.00 | Ent.cld | 7 | 7 | 6 | S. W. | 9 | 7 | 4 | 6 | 3 | 0 | |
| Minimum | 29.01 | 28.64 | 28.62 | 12.00 | 20.50 | 12.00 | 11.00 | 15.75 | −13.05 | 0.00 | 17.50 | Par. clr | 12 | 16 | 3 | S. E. | 1 | 3 | 7 | 0 | 0 | 0 | |
| Range | .86 | 1.15 | 1.15 | 47.00 | 49.50 | 50.00 | 48.00 | 48.75 | 75.14 | 41.05 | 47.50 | | | | | W. | 5 | 3 | 2 | 4 | 0 | 0 | |
| | | | | | | | | | | | | | | | | E. | 0 | 1 | 0 | 0 | 0 | 0 | |
| Omitted | 0 | 1 | 2 | 0 | 1 | 2 | 0 | 1 | 4 | 4 | 4 | | 0 | 1 | 2 | | 1 | 4 | 2 | 5 | 15 | 11 | 2.270 |
| MARCH. Att. ther. | 46.34 | 54.55 | 49.52 | | | | | | | | | | | | | N. | 0 | 1 | 2 | 0 | 0 | 1 | |
| | | | | | | | | | | | | | | | | N. W. | 10 | 12 | 5 | 6 | 12 | 3 | |
| | | | | | | | | | | | | | | | | N. E. | 4 | 2 | 1 | 0 | 0 | 0 | |
| Average | 29.50 | 29.48 | 29.50 | 41.18 | 54.66 | 45.32 | 37.89 | 46.53 | 39.40 | 14.86 | 47.59 | Ent. clr | 9 | 4 | 12 | S. | 4 | 1 | 0 | 1 | 1 | 0 | |
| Maximum | 29.90 | 29.90 | 29.78 | 60.00 | 75.00 | 63.00 | 65.00 | 64.25 | 65.00 | 34.33 | 65.00 | Ent.cld | 15 | 12 | 11 | S. W. | 9 | 12 | 5 | 2 | 6 | 0 | |
| Minimum | 29.15 | 29.00 | 29.00 | 24.00 | 37.00 | 31.00 | 22.50 | 29.75 | 12.67 | 0.00 | 30.00 | Par. clr | 7 | 13 | 8 | S. E. | 3 | 1 | 5 | 0 | 0 | 0 | |
| Range | .75 | .90 | 0.78 | 36.00 | 38.00 | 32.00 | 42.50 | 34.50 | 52.33 | 34.33 | 35.00 | | | | | W. | 1 | 0 | 6 | 3 | 2 | 0 | |
| | | | | | | | | | | | | | | | | E. | 0 | 0 | 1 | 0 | 1 | 0 | |
| Omitted | 0 | 2 | 0 | 0 | 2 | 0 | 0 | 2 | 2 | 2 | 2 | | 0 | 2 | 0 | | 0 | 2 | 6 | 10 | 6 | 15 | 1.760 |

| | | Barometer. | | | External Thermometer. | | | | | Hygrom'r, 2 P.M. | | | Sky. | | | Winds. | | | | Clouds. | | | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 7 A.M. | 2 P.M | 9 P.M. | 7 A.M. | 2 P.M. | 9 P.M. | Reg'r | Mean | Dew point | Diff. dw pt &tem | Wet bulb. | | 7 A.M Days | 2 P.M Days | 9 P.M Days | | 7 A.M Days | 2 P.M Days | 9 P.M Days | 7 A.M Days | 2 P.M Days | 9 P.M Days | Rain Gauge. Inches |
| April. | Att. ther. | 51.37 | 63.38 | 56.87 | | | | | | | | | | | | | N. | 2 | 2 | 2 | 1 | 1 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 6 | 2 | 4 | 3 | 8 | 0 | |
| | | | | | | | | | | | | | | | | | N. E. | 5 | 5 | 5 | 2 | 0 | 0 | |
| | Average | 29.47 | 29.44 | 29.44 | 48.82 | 62.86 | 53.83 | 45.45 | 53.93 | 46.51 | 16.69 | 53.84 | Ent. clr | 7 | 4 | 10 | S. | 0 | 2 | 0 | 0 | 0 | 0 | |
| | Maxim'm | 29.84 | 29.85 | 29.75 | 68.50 | 86.00 | 72.00 | 65.00 | 70.50 | 67.48 | 31.27 | 72.00 | Ent.cld | 11 | 10 | 15 | S. W. | 6 | 4 | 7 | 0 | 2 | 0 | |
| | Minimum | 29.05 | 29.01 | 28.97 | 29.00 | 45.00 | 40.00 | 28.00 | 37.00 | 27.51 | 0.00 | 39.00 | Par. clr | 12 | 15 | 5 | S. E. | 4 | 3 | 8 | 1 | 2 | 0 | |
| | Range | .79 | .84 | .78 | 39.50 | 41.00 | 32.00 | 37.00 | 33.50 | 39.97 | 31.27 | 33.00 | | | | | W. | 3 | 9 | 3 | 7 | 3 | 0 | |
| | | | | | | | | | | | | | | | | | E. | 4 | 2 | 1 | 2 | 3 | 0 | |
| Omitted | | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 1 | 1 | | 0 | 1 | 0 | | 0 | 1 | 0 | 7 | 7 | 20 | 3.840 |
| May. | Att. ther. | 56.19 | 67.48 | 61.89 | | | | | | | | | | | | | N. | 6 | 0 | 3 | 2 | 2 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 6 | 8 | 9 | 8 | 5 | 0 | |
| | | | | | | | | | | | | | | | | | N. E. | 4 | 2 | 3 | 1 | 1 | 0 | |
| | Average | 29.46 | 29.44 | 29.46 | 53.05 | 66.14 | 55.85 | 48.82 | 57.08 | 51.54 | 14.79 | 57.98 | Ent. clr | 7 | 3 | 13 | S. | 2 | 2 | 2 | 0 | 1 | 0 | |
| | Maxim'm | 29.76 | 29.75 | 29.76 | 67.00 | 80.00 | 65.50 | 63.50 | 68.50 | 70.84 | 27.73 | 71.50 | Ent.cld | 8 | 8 | 9 | S. W. | 11 | 7 | 7 | 6 | 4 | 0 | |
| | Minimum | 29.09 | 29.06 | 29.10 | 44.00 | 45.00 | 45.00 | 40.00 | 44.75 | 32.76 | 0.00 | 45.00 | Par. clr | 16 | 17 | 9 | S. E. | 0 | 2 | 4 | 0 | 0 | 0 | |
| | Range | .67 | .69 | .66 | 23.00 | 35.00 | 20.50 | 23.50 | 23.75 | 38.08 | 27.73 | 26.50 | | | | | W. | 2 | 5 | 3 | 4 | 7 | 1 | |
| | | | | | | | | | | | | | | | | | E. | 0 | 1 | 0 | 1 | 0 | 0 | |
| Omitted | | 0 | 3 | 0 | 0 | 3 | 0 | 0 | 3 | 4 | 4 | 4 | | 0 | 3 | 0 | | 0 | 4 | 0 | 4 | 8 | 17 | 3.710 |
| June. | Att. ther. | 64.90 | 74.88 | 68.15 | | | | | | | | | | | | | N. | 6 | 1 | 2 | 1 | 1 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 3 | 5 | 5 | 0 | 3 | 0 | |
| | | | | | | | | | | | | | | | | | N. E. | 3 | 1 | 5 | 1 | 0 | 0 | |
| | Average | 29.52 | 29.52 | 29.50 | 62.40 | 73.62 | 64.17 | 58.05 | 65.32 | 60.33 | 13.28 | 69.25 | Ent. clr | 5 | 5 | 9 | S. | 2 | 5 | 2 | 1 | 1 | 0 | |
| | Maxim'm | 29.87 | 29.84 | 29.86 | 75.50 | 88.00 | 79.00 | 68.50 | 76.00 | 74.93 | 23.41 | 76.00 | Ent.cld | 12 | 6 | 9 | S. W. | 6 | 5 | 4 | 7 | 9 | 0 | |
| | Minimum | 29.25 | 29.24 | 29.26 | 48.50 | 62.00 | 52.50 | 41.00 | 52.00 | 40.80 | 0.00 | 51.00 | Par. clr | 13 | 15 | 12 | S. E. | 7 | 4 | 8 | 1 | 2 | 0 | |
| | Range | .62 | .60 | .60 | 27.00 | 26.00 | 26.50 | 27.50 | 23.00 | 34.13 | 23.41 | 25.00 | | | | | W. | 3 | 3 | 4 | 13 | 7 | 0 | |
| | | | | | | | | | | | | | | | | | E. | 0 | 2 | 0 | 0 | 1 | 0 | |
| Omitted | | 0 | 4 | 0 | 0 | 4 | 0 | 0 | 4 | 4 | 4 | 4 | | 0 | 4 | 0 | | 0 | 4 | 0 | 4 | 4 | 21 | 4.670 |

*Meteorological Journal—Continued.*

| | | Barometer. | | | External Thermometer. | | | | | Hygrom'r, 2 P.M. | | | Sky. | | | | Winds. | | | | Clouds. | | | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 7 A.M. | 2 P.M. | 9 P.M. | 7 A.M. | 2 P.M. | 9 P.M. | Reg'r | Mean | Dew point | Diff. dw pt & tem | Wet bulb. | | 7 A.M Days | 2 P.M Days | 9 P.M Days | | 7 A.M Days | 2 P.M Days | 9 P.M Days | 7 A.M Days | 2 P.M Days | 9 P.M Days | Rain Gauge. Inches |
| July. | Att. ther. | 71.52 | 81.41 | 74.15 | | | | | | | | | | | | | N. | 5 | 1 | 3 | 0 | 1 | 1 | |
| | | | | | | | | | | | | | | | | | N. W. | 2 | 4 | 3 | 3 | 3 | 0 | |
| | | | | | | | | | | | | | | | | | N. E. | 5 | 4 | 3 | 2 | 3 | 0 | |
| | Average | 29.60 | 29.60 | 29.60 | 68.23 | 79.52 | 69.82 | 63.90 | 71.63 | 65.81 | 13.69 | 70.12 | Ent. clr | 9 | 2 | 12 | s. | 3 | 5 | 5 | 0 | 0 | 0 | |
| | Maxim'm | 29.83 | 29.85 | 29.79 | 75.50 | 88.50 | 77.50 | 72.00 | 79.50 | 74.51 | 20.92 | 78.00 | Ent.cld | 5 | 5 | 2 | s. W. | 10 | 8 | 9 | 5 | 9 | 0 | |
| | Minimum | 29.28 | 29.39 | 29.31 | 60.00 | 61.00 | 59.50 | 54.00 | 60.50 | 56.08 | 0.00 | 61.00 | Par. clr | 16 | 23 | 16 | s. E. | 2 | 1 | 5 | 1 | 0 | 0 | |
| | Range | .55 | .46 | .48 | 15.50 | 27.50 | 18.00 | 18.00 | 19.00 | 18.43 | 20.92 | 17.00 | | | | | W. | 1 | 2 | 1 | 7 | 8 | 0 | |
| | | | | | | | | | | | | | | | | | E. | 2 | 4 | 1 | 0 | 0 | 0 | |
| Omitted | | 1 | 2 | 1 | 1 | 2 | 1 | 0 | 3 | 2 | 2 | 2 | | 1 | 2 | 1 | | 1 | 2 | 1 | 4 | 6 | 18 | 4.105 |
| August. | Att. ther. | 68.03 | 78.81 | 71.90 | | | | | | | | | | | | | N. | 3 | 0 | 0 | 0 | 4 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 6 | 8 | 4 | 2 | 2 | 0 | |
| | | | | | | | | | | | | | | | | | N. E. | 6 | 5 | 6 | 1 | 2 | 0 | |
| | Average | 29.61 | 29.60 | 29.60 | 64.58 | 78.07 | 67.10 | 63.55 | 71.17 | 64.03 | 13.95 | 68.72 | Ent.clr | 7 | 2 | 11 | s. | 3 | 4 | 1 | 1 | 4 | 1 | |
| | Maxim'm | 29.89 | 29.88 | 29.86 | 71.00 | 84.00 | 74.00 | 71.00 | 77.00 | 69.85 | 21.52 | 73.00 | Ent.cld | 9 | 2 | 5 | s. W. | 1 | 4 | 2 | 3 | 5 | 2 | |
| | Minimum | 29.30 | 29.30 | 29.31 | 54.00 | 68.00 | 56.00 | 50.00 | 60.50 | 52.12 | 0.00 | 60.00 | Par. clr | 15 | 25 | 15 | s. E. | 6 | 5 | 11 | 3 | 5 | 0 | |
| | Range | .59 | .58 | .55 | 17.00 | 16.00 | 18.00 | 21.00 | 16.50 | 17.73 | 21.52 | 13.00 | | | | | W. | 3 | 1 | 3 | 6 | 5 | 2 | |
| | | | | | | | | | | | | | | | | | E. | 3 | 2 | 4 | 1 | 2 | 0 | |
| Omitted | | 0 | 2 | 0 | 0 | 2 | 0 | 0 | 2 | 2 | 2 | 2 | | 0 | 2 | 0 | | 0 | 2 | 0 | 7 | 1 | 16 | 3.330 |
| September. | Att. ther. | 61.42 | 73.31 | 63.95 | | | | | | | | | | | | | N. | 1 | 2 | 1 | 0 | 2 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 7 | 11 | 8 | 5 | 9 | 2 | |
| | | | | | | | | | | | | | | | | | N. E. | 7 | 2 | 6 | 1 | 0 | 0 | |
| | Average | 29.54 | 29.57 | 29.57 | 58.00 | 72.17 | 59.45 | 56.08 | 64.38 | 58.05 | 13.95 | 63.63 | Ent. clr | 8 | 4 | 11 | s. | 3 | 0 | 2 | 0 | 0 | 0 | |
| | Maxim'm | 29.77 | 29.78 | 29.76 | 73.00 | 89.00 | 76.00 | 73.00 | 80.00 | 73.16 | 19.07 | 76.00 | Ent.cld | 7 | 3 | 4 | s. W. | 9 | 4 | 4 | 2 | 3 | 0 | |
| | Minimum | 29.15 | 29.27 | 29.25 | 40 00 | 54.00 | 43.00 | 36.00 | 46.50 | 38.66 | 5.97 | 47.00 | Par. clr | 15 | 20 | 13 | s. E. | 0 | 3 | 3 | 1 | 2 | 0 | |
| | Range | .62 | .51 | .51 | 33.00 | 35.00 | 33.00 | 37.00 | 33.50 | 34.50 | 13.10 | 29.00 | | | | | W. | 2 | 5 | 3 | 9 | 7 | 0 | |
| | | | | | | | | | | | | | | | | | E. | 1 | 0 | 1 | 0 | 0 | 0 | |
| Omitted | | 0 | 3 | 2 | 0 | 3 | 2 | 0 | 3 | 3 | 3 | 3 | | 0 | 3 | 2 | | 0 | 3 | 2 | 5 | 4 | 17 | 1.345 |

| | | Barometer. | | | External Thermometer. | | | | | Hygrom'r, 2 P.M. | | | Sky. | | | | Winds. | | | Clouds. | | | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 7 A.M. | 2 P.M. | 9 P.M. | 7 A.M. | 2 P.M. | 9 P.M. | Reg'r | Mean | Dew point | Diff. dw pt & tem | Wet bulb. | | 7 A.M Days | 2 P.M Days | 9 P.M Days | | 7 A.M Days | 2 P.M Days | 9 P.M Days | 7 A.M Days | 2 P.M Days | 9 P.M Days | Rain Gauge. Inches |
| October. | Att. ther. | 48.00 | 61.44 | 53.35 | | | | | | | | | | | | | N. | 1 | 0 | 2 | 0 | 1 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 11 | 14 | 13 | 1 | 5 | 1 | |
| | | | | | | | | | | | | | | | | | N. E. | 2 | 1 | 1 | 0 | 0 | 0 | |
| | Average | 29.54 | 29.52 | 29.54 | 44.31 | 59.54 | 47.60 | 42.05 | 50.71 | 44.29 | 15.28 | 51.81 | Ent. clr | 20 | 11 | 22 | S. | 3 | 3 | 4 | 0 | 0 | 0 | |
| | Maxim'm | 29.89 | 29.88 | 29.83 | 60.00 | 70.50 | 58.50 | 56.00 | 61.25 | 54.99 | 27.73 | 60.00 | Ent. cld | 3 | 4 | 4 | S. W. | 2 | 4 | 1 | 5 | 5 | 0 | |
| | Minimum | 29.12 | 29.13 | 29.23 | 32.00 | 51.00 | 36.00 | 31.00 | 41.58 | 33.70 | 8.88 | 43.00 | Par. clr | 7 | 12 | 4 | S. E. | 4 | 2 | 2 | 0 | 0 | 0 | |
| | Range | .77 | .75 | .60 | 28.00 | 19.50 | 22.50 | 25.00 | 19.75 | 21.29 | 8.85 | 17.00 | | | | | W. | 6 | 1 | 5 | 1 | 3 | 1 | |
| | | | | | | | | | | | | | | | | | E. | 0 | 2 | 2 | 0 | 0 | 0 | |
| Omitted | | 2 | 4 | 1 | 2 | 4 | 1 | 0 | 6 | 4 | 4 | 4 | | 1 | 4 | 1 | | 2 | 4 | 1 | 4 | 7 | 7 | 2.040 |
| November. | Att. ther. | 36.40 | 44.08 | 39.32 | | | | | | | | | | | | | N. | 0 | 0 | 1 | 1 | 3 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 9 | 13 | 15 | 3 | 6 | 1 | |
| | | | | | | | | | | | | | | | | | N. E. | 4 | 3 | 3 | 0 | 0 | 0 | |
| | Average | 29.53 | 29.53 | 29.51 | 31.40 | 42.38 | 33.83 | 29.92 | 36.31 | 32.37 | 10.01 | 38.67 | Ent. clr | 13 | 5 | 16 | S. | 1 | 3 | 1 | 0 | 0 | 0 | |
| | Maxim'm | 29.89 | 29.88 | 29.85 | 50.00 | 60.00 | 50.00 | 49.00 | 48.00 | 53.50 | 19 62 | 54.00 | Ent. cld | 8 | 8 | 6 | S. W. | 2 | 1 | 1 | 2 | 6 | 0 | |
| | Minimum | 28.84 | 28.91 | 28.98 | 16.00 | 24.00 | 17.50 | 15.00 | 20.00 | 5.38 | 0.00 | 21.00 | Par. clr | 9 | 16 | 8 | S. E. | 3 | 3 | 3 | 0 | 0 | 0 | |
| | Range | 1.05 | .97 | .87 | 34.00 | 36.00 | 32.50 | 34.00 | 28.00 | 48.12 | 19.62 | 33.00 | | | | | W. | 10 | 6 | 6 | 4 | 1 | 4 | |
| | | | | | | | | | | | | | | | | | E. | 1 | 0 | 0 | 0 | 0 | 0 | |
| Omitted | | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 1 | 1 | | 0 | 1 | 0 | | 0 | 1 | 0 | 7 | 9 | 9 | 2.774 |
| December. | Att. ther. | 33.44 | 37.97 | 34.66 | | | | | | | | | | | | | N. | 1 | 2 | 1 | 2 | 1 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 15 | 15 | 16 | 6 | 11 | 4 | |
| | | | | | | | | | | | | | | | | | N. E. | 2 | 6 | 6 | 0 | 0 | 0 | |
| | Average | 29.50 | 29.46 | 29.49 | 28.11 | 35.23 | 28.74 | 25.85 | 30.54 | 25.44 | 9.72 | 32.21 | Ent. clr | 6 | 3 | 8 | S. | 2 | 2 | 0 | 0 | 0 | 0 | |
| | Maxim'm | 29,95 | 29.94 | 29.93 | 38.50 | 42.00 | 38.00 | 36.00 | 37.75 | 40.00 | 20.15 | 40 00 | Ent. cld | 11 | 12 | 13 | S. W. | 3 | 1 | 2 | 2 | 2 | 0 | |
| | Minimum | 28.77 | 28,90 | 28.81 | 11.00 | 19.50 | 13.50 | 11,00 | 16.75 | 7.35 | 0.00 | 18.50 | Par. clr | 14 | 16 | 10 | S. E. | 3 | 1 | 4 | 0 | 0 | 0 | |
| | Range | 1,18 | 1.04 | 1.12 | 27.50 | 22.50 | 24.50 | 25.00 | 21.00 | 32.65 | 20.15 | 21.50 | | | | | W. | 4 | 3 | 1 | 6 | 3 | 1 | |
| | | | | | | | | | | | | | | | | | E. | 1 | 1 | 1 | 0 | 0 | 0 | |
| Omitted | | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | | 0 | 0 | 0 | | 0 | 0 | 0 | 9 | 11 | 18 | 2.933 |

| | | *Average of* 1842. | | | | | | | | | | | | *Total of* 1842. | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Barometer. | | | External Thermometer. | | | | | Hygrom'r, 2 P.M. | | | Sky. | | | | Winds. | | | | Clouds. | | | Rain. |
| | | 7 A.M. | 2 P.M. | 9 P.M. | 7 A.M. | 2 P.M. | 9 P.M. | Reg'r | Mean | Dew point. | Diff. dw pt &tem | Wet bulb. | | 7 A.M Days | 2 P.M Days | 9 P.M Days | | 7 A.M Days | 2 P.M Days | 9 P.M Days | 7 A.M Days | 2 P.M Days | 9 P.M Days | Rain Gauge. Inches |
| 1842. | Att. ther. | 50.74 | 58.53 | 54.32 | | | | | | | | | | | | | N. | 26 | 9 | 22 | 7 | 16 | 2 | |
| | | | | | | | | | | | | | | | | | N. W. | 86 | 107 | 96 | 44 | 76 | 12 | |
| | | | | | | | | | | | | | | | | | N. E. | 52 | 35 | 45 | 8 | 6 | 0 | |
| | Average | 29.51 | 29.50 | 29.52 | 46.89 | 58.98 | 49.49 | 44.11 | 51.46 | 45.68 | 13.38 | 52.03 | Ent. clr | 109 | 52 | 154 | s. | 24 | 29 | 22 | 3 | 7 | 1 | |
| | Maxim'm | 30.02 | 29.94 | 29.96 | 75.50 | 89.00 | 79.00 | 73.00 | 80.00 | 74.93 | 41.05 | 78.00 | Ent.cld | 104 | 87 | 93 | s. W. | 79 | 69 | 52 | 42 | 58 | 2 | |
| | Minimum | 28.77 | 28.64 | 28.62 | 11.00 | 19.50 | 12.00 | 11.00 | 15.75 | -13.50 | 0.00 | 17.50 | Par. clr | 150 | 202 | 112 | s. E. | 38 | 32 | 66 | 7 | 11 | 0 | |
| | Range | 1.25 | 1.30 | 1.34 | 64.50 | 69.50 | 67.00 | 62.00 | 62.25 | 88.43 | 41.05 | 60.50 | | | | | W. | 44 | 41 | 39 | 71 | 54 | 11 | |
| | | | | | | | | | | | | | | | | | E. | 12 | 15 | 11 | 4 | 7 | 0 | |
| Omitted | | 3 | 24 | 6 | 3 | 24 | 6 | 0 | 24 | 29 | 29 | 29 | | 2 | 24 | 6 | | 4 | 28 | 12 | 74 | 86 | 184 | 33.497 |
| | | *Average of Winter Months*, 1841–42. | | | | | | | | | | | | *Total of Winter Months*, 1841–42. | | | | | | | | | |
| Winter, 1841–42. | Att. ther. | 34.91 | 41.42 | 38.25 | | | | | | | | | | | | | N. | 4 | 2 | 9 | 0 | 1 | 1 | |
| | | | | | | | | | | | | | | | | | N. W. | 15 | 23 | 21 | 11 | 18 | 3 | |
| | | | | | | | | | | | | | | | | | N. E. | 13 | 8 | 12 | 0 | 0 | 0 | |
| | Average | 29.41 | 29.44 | 29.44 | 30.78 | 40.90 | 33.99 | 28.64 | 34.51 | 30.52 | 10.78 | 35.66 | Ent. clr | 28 | 13 | 37 | s. | 3 | 4 | 8 | 0 | 0 | 0 | |
| | Maxim'm | 30.03 | 30.06 | 30.07 | 59.00 | 70.00 | 62.00 | 59.00 | 64.50 | 62.09 | 41.05 | 65.00 | Ent.cld | 28 | 29 | 28 | s. W. | 29 | 24 | 13 | 12 | 10 | 0 | |
| | Minimum | 28.80 | 28.64 | 28.62 | 10.50 | 20.50 | 12.00 | 10.50 | 15.75 | 13.05 | 0.00 | 17.50 | Par. clr | 34 | 45 | 23 | s. E. | 9 | 9 | 15 | 1 | 0 | 0 | |
| | Range | 1.23 | 1.42 | 1.43 | 48.50 | 49.50 | 50.00 | 48.50 | 48.75 | 75.14 | 41.05 | 47.50 | | | | | W. | 13 | 12 | 10 | 15 | 14 | 2 | |
| | | | | | | | | | | | | | | | | | E. | 3 | 2 | 0 | 0 | 1 | 0 | |
| Omitted | | 0 | 3 | 2 | 0 | 3 | 2 | 0 | 3 | 7 | 7 | 7 | | 0 | 3 | 2 | | 1 | 6 | 2 | 23 | 33 | 47 | 6.285 |
| | | *Average of Spring Months*, 1842. | | | | | | | | | | | | *Total of Spring Months*, 1842. | | | | | | | | | |
| Spring, 1842. | Att. ther. | 51.30 | 61.80 | 56.09 | | | | | | | | | | | | | N. | 8 | 3 | 7 | 3 | 3 | 1 | |
| | | | | | | | | | | | | | | | | | N. W. | 22 | 22 | 18 | 17 | 25 | 3 | |
| | | | | | | | | | | | | | | | | | N. E. | 13 | 9 | 9 | 3 | 1 | 0 | |
| | Average | 29.48 | 29.45 | 29.47 | 47.35 | 61.22 | 51.67 | 44.05 | 52.51 | 45.82 | 15.45 | 53.14 | Ent. clr | 23 | 11 | 35 | s. | 6 | 5 | 2 | 1 | 2 | 0 | |
| | Maxim'm | 29.90 | 29.90 | 29.78 | 68.50 | 86.00 | 72.00 | 65.00 | 70.50 | 70.84 | 34.33 | 72.00 | Ent.cld | 34 | 30 | 35 | s. W. | 26 | 23 | 19 | 8 | 12 | 0 | |
| | Minimum | 29.05 | 29.00 | 28.97 | 24.00 | 37.00 | 31.00 | 22.50 | 29.75 | 12.67 | 0.00 | 30.00 | Par. clr | 35 | 45 | 22 | s. E. | 7 | 6 | 17 | 1 | 2 | 0 | |
| | Range | .85 | .90 | .81 | 44.50 | 49.00 | 41.00 | 42.50 | 40.75 | 58.17 | 34.33 | 42.00 | | | | | W. | 6 | 14 | 12 | 14 | 11 | 1 | |
| | | | | | | | | | | | | | | | | | E. | 4 | 3 | 2 | 3 | 4 | 0 | |
| Omitted | | 0 | 6 | 0 | 0 | 6 | 0 | 0 | 6 | 7 | 7 | 7 | | 0 | 6 | 0 | | 0 | 7 | 6 | 21 | 21 | 52 | 9.310 |

*Average of Summer Months*, 1842.

| | | Barometer. | | | External Thermometer. | | | | | Hygrom'r, 2 P.M. | | | Sky. | | | | Winds. | | | | Clouds. | | | | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 7 A.M. | 2 P.M. | 9 P.M. | 7 A.M. | 2 P.M. | 9 P.M. | Reg'r | Mean | Dew point | Diff. dw pt &tem | Wet bulb | | 7 A.M Days | 2 P.M Days | 9 P.M Days | | 7 A.M Days | 2 P.M Days | 9 P.M Days | 7 A.M Days | 2 P.M Days | 9 P.M Days | Rain Gauge. Inches |
| Summer, 1842. | Att. ther. | 68.15 | 73.37 | 71.40 | | | | | | | | | | | | | N. | 14 | 2 | 5 | 1 | 6 | 1 | |
| | | | | | | | | | | | | | | | | | N. W. | 11 | 17 | 12 | 5 | 8 | 0 | |
| | | | | | | | | | | | | | | | | | N. E. | 14 | 10 | 14 | 4 | 5 | 0 | |
| | Average | 29.58 | 29.57 | 29.57 | 65.07 | 77.07 | 67.03 | 61.83 | 69.37 | 63.39 | 13.64 | 69.36 | Ent. clr | 21 | 9 | 32 | S. | 8 | 14 | 8 | 2 | 5 | 1 | |
| | Maxim'm | 29.89 | 29.88 | 29.86 | 75 50 | 88.50 | 79.00 | 72.00 | 79.50 | 74.93 | 23.41 | 78.00 | Ent. cld | 26 | 13 | 16 | S. W. | 7 | 17 | 15 | 15 | 23 | 2 | |
| | Minimum | 29.25 | 29.24 | 29.26 | 48.50 | 61.00 | 52.50 | 41.00 | 52.00 | 40.80 | 0.00 | 51.00 | Par. clr | 44 | 62 | 43 | S. E. | 15 | 10 | 24 | 5 | 7 | 0 | |
| | Range | .64 | .64 | .60 | 27.00 | 27.50 | 26.50 | 31.00 | 27.50 | 34.13 | 23.41 | 27.00 | | | | | W. | 7 | 6 | 8 | 26 | 20 | 2 | |
| | | | | | | | | | | | | | | | | | E. | 5 | 8 | 5 | 1 | 3 | 0 | |
| Omitted | | 1 | 8 | 1 | 1 | 8 | 1 | 0 | 9 | 8 | 8 | 8 | | 1 | 8 | 1 | | 1 | 8 | 1 | 15 | 11 | 55 | 12.105 |

*Average of Autumnal Months*, 1842.

| | | | | | | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Autumn, 1842. | Att. ther. | 48.61 | 59.68 | 52.21 | | | | | | | | | | | | | N. | 2 | 2 | 4 | 1 | 6 | 0 | |
| | | | | | | | | | | | | | | | | | N. W. | 27 | 38 | 36 | 9 | 20 | 4 | |
| | | | | | | | | | | | | | | | | | N. E. | 13 | 6 | 10 | 1 | 0 | 0 | |
| | Average | 29.54 | 29.54 | 29.54 | 44.57 | 58.03 | 46.96 | 42.68 | 50.47 | 44.90 | 13.08 | 51.37 | Ent. clr | 41 | 20 | 49 | S. | 7 | 6 | 7 | 0 | 0 | 0 | |
| | Maxim'm | 29.89 | 29.88 | 29.85 | 73.00 | 89.00 | 76.00 | 73.00 | 80.00 | 73.16 | 27.73 | 76.00 | Ent. cld | 18 | 15 | 14 | S. W. | 13 | 9 | 6 | 9 | 14 | 0 | |
| | Minimum | 28.84 | 28.91 | 28.98 | 16.00 | 24.00 | 17.50 | 15.00 | 20.00 | 5.38 | 0.00 | 21.00 | Par. clr | 31 | 48 | 25 | S. E. | 7 | 8 | 8 | 1 | 2 | 0 | |
| | Range | 1.05 | .97 | .87 | 57.00 | 65.00 | 58.50 | 58.00 | 60.00 | 67.78 | 27.73 | 55.00 | | | | | W. | 18 | 12 | 14 | 14 | 11 | 5 | |
| | | | | | | | | | | | | | | | | | E. | 2 | 2 | 3 | 0 | 0 | 0 | |
| Omitted | | 2 | 8 | 3 | 2 | 8 | 3 | 0 | 10 | 8 | 8 | 8 | | 1 | 8 | 3 | | 2 | 8 | 3 | 16 | 20 | 33 | 6.159 |

*Total of Summer Months*, 1842

*Total of Autumnal Months*, 1842.

## Practical & Theoretical Mechanics & Chemistry.

*On the Comparative Expense of Light derived from different sources, and on the Use of Chlorine as an Indication of the Illuminating Power of Coal Gas. By* ANDREW FYFE, M.D., F.R.S.E., F.R.S.S.A., &c.

In a paper published in the Edinburgh Philosophical Journal for 1824, I recommended the condensation of the heavy hydro-carbons by chlorine, as an easy and efficacious method of ascertaining the comparative illuminating power of coal gas, while, at the same time, it had the advantage of enabling us to compare one gas with another, though not brought directly into contrast with it, and thus, by fixing on one as a standard, to state the illuminating power numerically.

With regard to the methods now in use, I mean the specific gravity, the quantity of oxygen necessary for combustion, and the depth of shadow, the last is the only one in which we can place any confidence. As to the specific gravity, if the gas be pure, that is, free from carbonic acid and sulphuretted hydrogen, then, the heavier it is, the more likely is it to be of high illuminating power; but this is not always the case: thus, the specific gravity of olefiant gas and of carbonic oxide is the same, but the latter burns with a feeble blue flame, whereas the former gives forth a brilliant light. Now, suppose coal gas to contain little of the heavy hydro-carbon, and a large proportion of carbonic oxide, then the specific gravity may be such as to induce us to expect the illuminating power to be high, when in fact it is not.

The same remark is applicable to the mode of testing by the quantity of oxygen necessary for complete combustion. A gas with much olefiant will no doubt require much oxygen, this gas taking no less than thrice its own bulk; but let us suppose a variety of gases to have the same proportion of olefiant, or of heavy hydro-carbons, while the proportion of the other inflammable gases varies, which, though they consume oxygen, give out little light during their combustion, and we shall find that the amount of oxygen required gives no indication whatever of the illuminating power.

Thus, suppose the composition to be—

| | | | |
|---|---|---|---|
| Olefiant, - - - - - - | 13 | 13 | 13 |
| Carburetted hydrogen, - - - | 83 | 65 | 51 |
| Carbonic oxide, - - - - | 4 | 14 | 8 |
| Hydrogen, - - - - - | 0 | 8 | 28 |
| | 100 | 100 | 100 |

the first would require 207, the second 180, the third 159, of oxygen, yet the illuminating power would be nearly the same in all. Supposing the heavy hydro-carbons to vary, and even to become considerable, yet the quantity of oxygen may not be in proportion, owing to

the hydrogen and carbonic oxide, which require only half of their bulk of that gas for combustion. The mode of ascertaining the illuminating power by the shadow is one in which we may place the utmost reliance, provided we burn the gases with the same kind of burners, and pay particular attention to the circumstances affecting the appearance of the shadow; for it is well known that the color of the shadow varies even from the same gas, when the flames from different burners are contrasted; besides, the reflection of light from surrounding objects will also occasion a difference. Great care is therefore necessary when conducting the trials in this way; and it requires nicely adjusted meters, and a regular pressure, so that the consumption shall not vary during the performance of the experiment.

The other method which I formerly recommended is not liable to these fallacies. In the paper to which I have already alluded, the results of numerous trials are given, in which the illuminating power, as shown by the chlorine test, very nearly agrees with those indicated by the photometric process; and these experiments were performed with every possible attention to the circumstances likely to affect the results, *so far as they were then known.* In a paper subsequently published by Drs. Christison and Turner, the accuracy of the chlorine test was called in question, partly because, when testing the gases by the photometric process, as pointed out by Rumford, due attention was not paid to the different circumstances affecting the combustion, and partly owing to the opinion expressed in the paper by the authors, that other ingredients than olefiant exist in coal gas, which afford light by their combustion, and which are also condensible by chlorine. As to the latter objection, it is of little value, provided we find the results indicated by the chlorine test, to agree with the photometric one. With regard to the latter, it must be admitted that, in some trials, where two gases were compared with each other, due attention was not paid to the height of the flame, and to the other circumstances affecting the combustion, which, at the time that I was engaged in the inquiry, were not known to have an influence on the illuminating power. The influence of these has now been fully investigated, and made known, in the paper by Drs. Christison and Turner, and also in that which I read to the Society in 1840. Since then, I have again had my attention drawn to the subject, and have had many opportunities of putting the chlorine method to the test of experiment; and I must say that I am more and more inclined to put the most implicit confidence in it, not only as a very simple, but, at the same time, a correct, method of ascertaining the comparative illuminating power. I trust the results of the trials will not be devoid of interest.

In fixing the illuminating power of the gases by the shadow, two accurately adjusted meters were used, one for the one gas, the other for the other. Sometimes the gases were contrasted with each other; in which case similar burners, consuming the gas under the same circumstances, were employed; and with the view of securing accuracy in the results, the burners were sometimes changed from one gas to

another; at other times, the light given by the gas was contrasted with that from candles. The gases subjected to trial were sometimes those with which Edinburgh is at present supplied; sometimes they were prepared by myself, in a small apparatus, with the view of having the illuminating power as varied as I could possibly obtain.

It is well known that the quality of coal gas, even when manufactured from the same kind of coal, depends much on the mode of manufacture; when slowly prepared, and when the same charge of coal is long subjected to heat, a larger quantity of gas is given off than when the time for the charge is shorter; but then the illuminating power is low, owing to the gas which is last evolved having very little of the heavy hydro-carbons; and hence those companies who dispose of their coke to advantage, have, besides the quantity of gas to be got, another object in view, viz., the freeing of the coke from all its gaseous ingredients, otherwise it will not be considered valuable, indeed will not be purchased by those in the custom of using it. It is this which, in addition to the difference in the quality of the coal employed, makes such a difference between the quality of gas prepared in England and Scotland; for, as the coke from English caking coal is more prized than that from parrot coal, which is much used in Scotland, the English companies may generally be considered not only as gas companies, but also as coke companies—indeed, they derive a great deal of their profit from the coke. Hence, in judging of the price of gas, we must take into account its quality; and hence, I conceive, the importance of having an easy method of ascertaining this, and of comparing different gases with each other.

In the first series of experiments, the results of which I am now to give, two gases, manufactured under different circumstances, were compared with the light afforded by a wax candle, kept burning, as nearly as possible, with a uniform flame; the gases being consumed in jet burners with a 5 inch flame. Taking the average of several trials, gas A gave a light as 2.16, compared to that of the wax candle as 1; the condensation by chlorine was 15. Gas B, under similar circumstances, gave a light as 1.98; condensation by chlorine 13, and 15 : 13 : : 2.16 : 1.86; by the shadow it was 1.98.

In another trial with other gases, the light was compared with that afforded by a *tallow* candle, (short six.) Gas C, the light was as 2.81, to that of the candle as 1; condensation by chlorine, 15. Gas D, the light was 2.27, chlorine test 12;

and as 2.81 : 2.27 : : 11 : 8.02,
and as 15 : 13 : : 1 : 8.00,

which is a very close approximation.

Two gases were next contrasted with each other, being consumed with fish-tail burners. By the shadow, the light for equal consumption was 1 to .827, by the chlorine 14 : 12, and as 14 : 12 : : 1 : .857. In another trial with the same burners, but with gases prepared at another time, the average of numerous trials by the photometric process, gave the result as 1 to .945; condensation by chlorine was 12.5 and 11.5, and as 12.5 : 11.5 : : 1 : .92.

With jets, and with other gases, the results were, by the shadow, 1 to 1.185, and, by chlorine, 11 to 14, and 11 : 14 : : 1 : 1.272. Here the approximation is not so close as in some of the others.

The chlorine test was then tried with a gas, the illuminating power of which was inferior to that of the preceding. The trial by the shadow was made at different distances, to secure accuracy. By the one the result was as 1 to 1.347, by the other to 1.338, average 1 to 1.342. The condensation by chlorine was 10 and 14, which very nearly coincides with the others.

The results above stated very nearly agree with each other. In one trial, however, I found that they did not come so close. By the shadow they were 1 to 1.33, by the chlorine 11 to 17, now as 11 : 17 : : 1 : 1.54.

In this instance the discordance may, I think, be accounted for. It is well known that, when the illuminating power of a gas is high, as when it is prepared by the decomposition of oil, it requires a burner with smaller apertures than those used for common coal gas, otherwise it is not consumed to advantage. Now, in the experiment last recorded, in which the condensation by chlorine amounted to 17, a coal-gas jet was used, by which the gas would not give the same amount of light that it would have given, had a burner with smaller apertures been employed. Hence the illuminating power indicated by the shadow does not come up to what, most likely, it would have been with a differently constructed burner. May not this exception prove the accuracy of the proposed test?

From what has now been said with regard to the test which I have proposed, I think we are warranted in placing implicit confidence in it, as a means of indicating the illuminating power of coal gas; indeed, I have no hesitation in stating, that, when the trial is properly conducted, it leads to results more satisfactory than those given by the shadow; for it has this advantage, that, while it is much more easily conducted, it points out the amount of light that ought to be afforded by one gas as compared with another; whereas, unless all the different circumstances that affect the combustion of the gases are attended to, the results by the shadow will not be correct. One of them, in particular, is the kind of burner—for, when gas is rich in matter condensible by chlorine, and a common coal-gas burner is used, the illuminating power indicated by the shadow will, most probably, be below what it really is, owing to the burner not being adapted for the combustion of that peculiar kind of gas; and hence one of the advantages of the chlorine test.

The process practised in the experiments I have detailed is, with a slight modification, the same as that formerly described. Two tubes, of about half an inch in diameter, and 12 inches long, of the same calibre, and graduated to 100 parts, are employed; into the one, 50 degrees of the gas under investigation are introduced, and afterwards into the other there are put 50 of chlorine; the water of the trough being heated to 50, or thereabouts. The coal gas is then transferred into the chlorine, and the tube instantly covered with a shade, to prevent the action of the light. In the course of five minutes, the con-

densation is complete. Should only one graduated tube be used, the coal gas must be measured first, and then put into another tube, after which the chlorine is measured, and the *coal gas transferred* into it; for, if otherwise, a part of the chlorine would be absorbed by the water, during its passage through it, and thus lead to variable results. As chlorine is absorbable by water, a slight absorption takes place during the continuance of the experiment. Before proceeding to any trials, it is therefore necessary to ascertain the amount of this, and then to deduct it from the condensation occasioned by the action on the coal gas. In the tube which I have used, I found the absorption to be exactly one degree for every five minutes, and which continues in the same ratio, after the action of the chlorine on the hydro-carbon is over. I have, therefore, always deducted one degree for each five minutes from the total loss, as indicated by the rise of the water in the tube. As, however, the action is over in five minutes, I have seldom continued the trial beyond that time, of course deducting one degree from the loss sustained. As chlorine and the condensible matter act on each other in equal volumes, a condensation of 10, when 50 of each are used, indicates ten per cent. of loss by the coal gas.

Should this method of ascertaining the illuminating power of gases be ultimately found to be correct, another important result may follow its introduction into practice. If we can, by it, fix the illuminating power of one gas compared with that of another gas, the quality of which has been previously determined, and which is consumed with a burner that is known to burn it advantageously, and if the gas which we are subjecting to trial by the shadow test does not show such a high illuminating power as we are led to expect, from the known condensation by chlorine, the probability is, that the burners are not adapted for consuming the gas advantageously, and hence the necessity of altering the apertures, till the power by the shadow is what it ought to be, according to the chlorine test.

There is still another advantage attending the introduction of the chlorine test, in addition to those mentioned; it is the facility of comparing different gases with one another, when they cannot be brought together so as to try them by the shadow. The illuminating power may be considered just as the condensation by chlorine, and thus, then, we may state it *numerically*. Thus, taking a coal gas having only one per cent. of matter condensible by chlorine, its illuminating power may be considered as *unity*, and all others would be as *the per centage of condensation*. Hence, also, the illuminating power of gases may be ascertained, as compared with other sources of light.

It is evident, from what has been said, that, in finding the value of a gas as compared with other sources of light, attention must be paid to the *quality* of the gas; a circumstance which, by many, has been totally disregarded, and hence the very discordant results which have been obtained. In comparing the gas by the shadow given by other lights, we must, in fact, not only attend to the different circumstances affecting the combustion; we must also, at each trial, ascertain the amount of condensation by chlorine; for the quality of a gas manu-

factured on different days, at the same place, will be found to vary considerably. In the trials I am now to state, made with the view of finding the comparative expense of light, as got from candles, oil, &c., I have uniformly kept this in view; and, by doing so, we are enabled to judge of the expense, not only in this town, but also in other places, provided, of course, we know the illuminating power of the gas by the chlorine test.

The first series of experiments were those with candles, of which ten different kinds were tried. Tallow single wick, tallow double wick, cocoa, palm, composite, margerine, diaphane, composition, spermaceti, wax,—all short sixes.

*Tallow.*—Very different statements have been given of the illuminating power of coal gas, as compared with that from tallow candles, and which has been accounted for by the difficulty of getting the light from the candle to be uniform; the chief cause of the discordance is, however, more probably, the difference in the quality of the gases manufactured at different places. In conducting my trials, I have paid due attention to the former, trying the candles at different times, so as to have a wick of various lengths. The standard gas light, in all the trials, was a jet burning under a uniform pressure, with a flame of five inches, and consuming exactly one foot per hour.

From numerous trials, I found that the tallow, (single wick, short six,) when compared with the gas, and taking the average of all the trials, was as 1 to 3.75. A short six will be found, when properly snuffed, to last for six hours, or very nearly so; and supposing candles to be 7½ per pound, then the cost of each candle is 5 farthings. Suppose the gas to cost 8s. 4d. per 1000 feet,* then six feet will cost 2½ farthings, or very nearly so; accordingly, for half the expense, 3.75 times the amount of light is obtained; in other words, for the same light, the expense of tallow candles is 7½ times that of gas. The gas I employed in these trials contained, on an average, 12 per cent. of condensible matter. Should the gas contain more or less, then the comparative expense would be greater or less, just according to the quantity. In Edinburgh I have found the chlorine test to indicate from 11 to 14 and 15, very rarely is it up to the latter; of late, I have rarely found it to go beyond 13. Considering the foregoing calculation as applying to the gas now supplied to Edinburgh, and presuming it to contain 12 per cent. of matter condensible by chlorine, then the expense of tallow candles is 7½ times greater, *for the same light*, than that of gas consumed by jet burners.

In England, where the gas is generally manufactured from English caking coal, the illuminating power is inferior to that of gas got from parrot coal, or from a mixture of it and common Scotch coal. Now, suppose the price of the gas the same, and that the condensation by chlorine amounts to 6, then the comparative expense of candles and of gas for the same light would be 3.75 to 1.

Similar trials were made with the other candles mentioned.

* I have taken this as being easy for calculation. It is not far from the price of gas in Edinburgh, and in other towns in the neighborhood of the coal districts.

***Double-wicked tallow***, 1s. per pound.—This candle burns for 5¼ hours, at a cost of 8 farthings; the light, compared to that of the jet, is as 1 to 2.1, making the expense as 7.1 to 1. This candle has the advantage of not requiring to be snuffed.

*Cocoa* candle, 11d. per pound, will burn for nine hours, at a cost of 7.3 farthings; the light, compared to the jet, is as 1 to 3.6, or the same as that of the common tallow candle, thus making the expense as 7.3 to 1.

*Palm* candle, 1s. 2d. per pound, will burn for 6.6 hours, expense 9.3 farthings, light 1 to 3, expense as 10.5 to 1.

*Composite*, 1s. 1d. per pound, lasts for eight hours, expense 8.6 farthings, light 1 to 3, expense 8 to 1.

*Diaphane*, (French,) 1s. 8d., will last 6.6 hours, at a cost of 12.3 farthings, light 1 to 3, expense 15.1 to 1.

*Margarine*, nearly, in every respect, the same as diaphane.

*Spermaceti*, 2s. 6d. per pound, will burn for eight hours, cost 20 farthings, light 1 to 2.6, expense as 16.2 to 1.

*Composition* candle the same.

*Wax*, 2s. 6d., burns nine hours, cost 20 farthings, light as 1 to 2.6; expense, therefore, as 14.4 to 1.

Thus the *tallows*, with the exception of the palm, are nearly of the same comparative expense, light for light; the *composition* is a very little more expensive—the others are more than double the expense.

In the foregoing calculations, I have supposed the gas to be consumed by jets; but I have already shown, in the paper read before the Royal Scottish Society of Arts, and published in its Transactions for 1840, that this is the least profitable method of burning it. For *equal consumpts*, the light given by other burners is much greater; thus, taking the jet as 100, that from a fish-tail is 140, from the bat-wing 160, and from a properly constructed argand 180. Accordingly, by consuming the gas with these, the comparative expense will be still farther reduced. The following table gives the comparative light and expense, according to the kind of burner used.

In conducting the experiments with the view of ascertaining the illuminating power of oil, compared with that of gas, I used argand oil-lamps of the common construction, and also others with contrivances adapted to them, which have lately been recommended for increasing the light. The first trials were made with *sperm* oil, the cost of which, at the time the trials were made, was 9s. 8d. per gallon, that is, 1s. 2½d. per pint. It was burned in a common argand, consuming the oil under the most favorable circumstances. In endeavoring to fix the illuminating power, I contrasted it with an argand gas burner, having forty-two holes, and consuming three feet per hour. I found, however, considerable difficulty in coming to accurate results, partly from the variation in the flame of the oil, partly also from the difference in the appearance of the shadow. Six trials were made at different times, and with the lights at different distances. These varied from 2 to 2.4, taking the oil as 1. The average of the different trials gave 2.35. A pint of oil was found to burn 14 hours, at a cost of 14½d.; the consumpt of gas for the same time (3 × 14) was 42 feet,

| Candles. Short Sizes. | Burns hours. | Light compared with Jet. Candle 1. | Cost in farthings. Candle | Gas | Expense of Candle compared with Jet. Candle. | Gas. | Light comp'd with Fish-tail. Candle | Gas | Expense compared with Fish-tail. Candle | Gas | Light compared with Argand. Candle | Gas | Expense com'd with Argand. Cand. | Gas | Compt've Expense of candles for equal light. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. Tallow. Single wick | 6 | 1 to 3.6 | 5 | 2.4 | 7.5 to | 1 | 1 | 5. | 10.5 | 1 | 1 | 6.48 | 13.5 | 1 | 1.0 |
| 2. Tallow. Double wick | 5.5 | 1...2.1 | 8 | 2.2 | 7.1 .. | 1 | 1 | 2.9 | 9.94 | 1 | 1 | 3.76 | 12.78 | 1 | 1.46 |
| 3. Cocoa | 9 | 1...3.6 | 7.33 | 3.6 | 7.33.. | 1 | 1 | 5. | 10.22 | 1 | 1 | 6.18 | 13.5 | 1 | 1.0 |
| 4. Palm | 6.6 | 1...3 | 9.33 | 2.6 | 10.5 .. | 1 | 1 | 4.2 | 14.70 | 1 | 1 | 5.40 | 18.90 | 1 | 1.32 |
| 5. Composite | 8 | 1...3 | 8.66 | 3.2 | 8.12.. | 1 | 1 | 4.2 | 11.34 | 1 | 1 | 5.4 | 13.18 | 1 | 1.1 |
| 6. Diaphane | 6.6 | 1...3 | 13.33 | 2.4 | 15.1 .. | 1 | 1 | 4.2 | 21.14 | 1 | 1 | 5.4 | 14.18 | 1 | 2.08 |
| 7. Margerine | 6 | 1...3 | 13.33 | 2.4 | 15.6 .. | 1 | 1 | 4.2 | 22.68 | 1 | 1 | 5.4 | 27.5 | 1 | 2.15 |
| 8. Spermaceti | 8 | 1...2.6 | 20 | 3.2 | 16.2 .. | 1 | 1 | 3.64 | 22.7 | 1 | 1 | 4.86 | 28.44 | 1 | 2.16 |
| 9. Composition | 8 | 1...2.6 | 20 | 3.2 | 16.2 .. | 1 | 1 | 3.64 | 22.7 | 1 | 1 | 4.86 | 29.2 | 1 | 2.16 |
| 10. Wax | 9 | 1...2.6 | 20 | 3.6 | 14.4 .. | 1 | 1 | 3.64 | 20.16 | 1 | 1 | 4.86 | 25.92 | 1 | 1.96 |

In the above calculations, no allowance is made for outlay in gas fittings, &c.

at an expense of 4¼d., but the light was as 2.25 to 1. The comparative expense, therefore, light for light, would be as 14½d. × 2.25 to 4¼d.; that is, as 8 to 1, or very nearly so.

Rectified *whale* oil was next tried, the cost of which was 4s. 8d. per gallon. A pint, when consumed under the most favorable circumstances, was found to burn 12 hours, and, contrasted with the gas argand, as before, the light was as 1 to 2.54. The cost of oil was 7d., that of gas for the same time was 3½d., but the light was as 1 to 2.54: the expense was, therefore, for the same light, as 7d. × 2.54 to 3½d.; that is, 5 to 1.

In the preceding trials, the oil was consumed in a common argand, due attention being paid to the different circumstances affecting the consumpt, such as the kind of wick, the height of flame, &c. The next trial was made with the lamp lately introduced under the name of *solar lamp*. In this, a cylinder surrounds that containing the wick, with the upper part bent inwards, so that, the aperture being contracted, the current of air that passes up between the one cylinder and the other, striking against the horizontal part of the outer one, causes a contraction and lengthening of the flame; a longer and narrower glass chimney is at the same time required. The advantages said to attend the use of this construction of burner are, that an oil of inferior quality may be used, while, at the same time, the light is greatly increased.

The solar lamp, containing *solar oil*, with a flame as high as could be got to be steady, and without smoke, was contrasted with the gas argand as before, burning three feet per hour. On comparing the lights, and taking the average of numerous trials, conducted at different distances, and when the wick was in different conditions, they were as 0.98 to 1; so very nearly equal, that we may consider them as so. The oil, per gallon, costs 3s. 8d.; a pint was found to burn eight hours, or very nearly so, at a cost of 5½d. The gas required for the same time is 24, or say 25, feet, which would cost 2½d; accordingly, the expense is rather more than twice, or say twice, that of the gas.

To ascertain whether or not there is any saving by using the apparatus adapted to the solar lamp, the solar oil was consumed with a solar wick, in the same argand with which the trials with the sperm and whale oils were made; and the light, as before, was contrasted with the argand, burning three feet per hour. The light and the consumpt of oil were found to be the same as with the other oils. The cost of the solar oil per pint is 5½d., that of the whale oil 7d.; accordingly, the expense is as the cost of the oils. It has been already stated, that, by using the solar apparatus, the oil gave a light equal to that from an argand consuming three feet per hour, and that the pint of oil will last for eight hours; the expense is, therefore, as 2¼d. to 5½d., or say 1d. to 2d. Now, when the solar oil was burned in the common argand, and contrasted with the gas argand, the light was as 1 to 2.54. As the oil lasted for twelve hours, the cost of gas for that time would be 3½d., or very nearly so. The comparative expense was, therefore, as 5½d. × 2.54 to 3½d.; that is, as 3.98 to 1; whereas,

by the solar lamp, it was only as 2 to 1; thus making a saving, by the use of the solar lamp, of nearly one-half of the expense. This peculiar construction of lamp is, therefore, a very great improvement; for not only is there a saving in expense in the outlay for oil, but, for the lighting of large apartments, a smaller number of lamps is required than when common argands are employed.

*Naphtha.*—This article has been lately recommended as an economical source of light. Though naphtha gives a beautiful and steady light, yet it emits an offensive smell, and, unless cautiously burned, is very liable to smoke, the slightest blast against the flame causing dense black smoke instantly to appear. The appearance of the shadow is so different from that from coal gas, that it is not easy to fix their illuminating power, and consequent comparative expense. In the experiments I have performed, I used the gas argand as before, consuming four feet per hour. The naphtha lamp had a wick of four inches in breadth, and burned with a flame of about half an inch in height. In one trial, I made the illuminating power of the flames, as naphtha 1 to gas 4.33; in another, they were as 1 to 4.239; giving an average of 1 to 4.236. The consumpt of naphtha was a pint in 24 hours, at a cost of 3s. 6d. per gallon; that is, 5¼d. per pint. The gas for the same time would be 24, or say $25 \times 4 = 100$; that is, 10d.; but the light was as 4.236 to 1—therefore the comparative expense comes to be as 2.2 to 1, or very nearly so. Suppose that I have overrated the illuminating power of the gas, as compared with that of the naphtha,—say that, instead of 4.236, it was about 4,—this would reduce the cost of the latter, and thus make the comparative expense as about 2 to 1.

*Table showing the Consumption and Expense of Oils, and of Gas, in Argands, burning three feet per hour.*

| Oils per Pint. | Pint burns hours | Light of gas com'd with oils as 1. | Cost in farthings of | | Comparative Expense for equal lights. | | Comparative Expense of Oils for equal lights. |
|---|---|---|---|---|---|---|---|
| | | | Gas | Oil | Gas | Oil | |
| Sperm in Argand..... | 14 | 2.35 | 17 | 58 | 1 | 8 | 4 |
| Whale do. ..... | 12 | 2.54 | 14 | 28 | 1 | 5 | 2.5 |
| Solar do. ..... | 12 | 2.51 | 14 | 22 | 1 | 3.98 | 1.99 |
| Solar in Solar lamp..... | 8 | 1 | 10 | 22 | 1 | 2 | 1 |
| Naphtha lamp..... | 24 | 3.17 | 40 | 21 | 1 | 2 | 1 |

***Table showing the comparative Expense of Light from different sources; Coal Gas, containing 12 per cent. of matter condensible by Chlorine, taken as unity.***

| | | | | | |
|---|---|---|---|---|---|
| Argand Gas, | 1.00 | | | | |
| Fish Tail, | 1.40 | Fish Tail | 1.00 | | |
| Single Jet, | 1.80 | " | 1.40 | Jet, | 1.00 |
| Solar Lamp, | 2.00 | " | 1.55 | " | 1.11 |
| Naphtha, | 2.00 | " | 1.55 | " | 1.11 |
| Solar oil in com'n Argand, | 3.98 | " | 2.84 | " | 2.21 |
| Whale oil do. | 5.00 | " | 3.88 | " | 2.77 |
| Sperm do. | 8.00 | " | 6.22 | " | 4.41 |
| Tallow candle, (2 wicks,) | 12.7 | " | 10.0 | " | 7.18 |
| Cocoa candle, | 13.1 | " | 10.2 | " | 7.33 |
| Tallow do. (1 wick,) | 13.5 | " | 10.5 | " | 7.50 |
| Composite, | 14.5 | " | 11.3 | " | 8.12 |
| Palm, | 18.9 | " | 14.7 | " | 10.5 |
| Wax, | 25.9 | " | 20.1 | " | 14.4 |
| Diaphane, | 27.1 | " | 21.1 | " | 15.1 |
| Margarine, | 28.4 | " | 22.6 | " | 15.6 |
| Spermaceti, | 29.2 | " | 22.7 | " | 16.2 |
| Composition, | 29.2 | " | 22.7 | " | 16.2 |

Lond. Mech. Mag.

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### *The great Explosion at Dover.*

Having witnessed the great explosion at Dover, on Thursday the 26th, from the summit of the cliff next adjoining it to the southward, and from the nearest point to which any access was permitted, I would gladly place on record, in your valuable Journal, some features of this magnificent operation, which struck me at the time as extremely remarkable, and which have not, I think, been adequately placed before the public in any account that I have seen. These features are, the singular and almost total absence of all those tumultuous and noisy manifestations of power which might naturally be expected to accompany the explosion of so enormous a quantity (19,000 lb.) of gunpowder, and which formed, I have no doubt, the chief attraction of many who came from great distances to witness it, viz.—noise, smoke, earthquake, and fragments hurled to vast distances through the air.

Of the noise accompanying the immediate explosion, I can only describe it as a low murmur, lasting hardly more than half a second, and so faint, that had a companion at my elbow been speaking in an ordinary tone of voice, I doubt not it would have passed unheeded. Nor was the fall of the cliff (nearly 400 feet in height, and of which no less than 400,000 cubic yards were, within an interval of time hardly exceeding ten seconds, distributed over the beach on an area of eighteen acres, covered to an *average* depth of 14 feet, and in many parts from 30 to 50,) accompanied with any

considerable noise; certainly with none which attracted my own attention, or that of several others similarly stationed, with whom I afterwards compared notes. A pretty fresh breeze from the south-west might be regarded as influential in wafting it away, were it not that the fall took place under the lee of the cliff on whose edge we were stationed.

The entire absence of *smoke* was another and not less remarkable feature of the phenomenon. Much *dust*, indeed, curled out *at the borders* of the vast rolling and undulating mass, which spread itself like a simi-fluid body, thinning out in its progress; but this subsided instantly; and of *true smoke* there was absolutely not a vestige. Every part of the surface was immediately and clearly seen—the *prostrate* flagstaff* (*speedily re-erected in the place of its fall*)—the broken turf which a few seconds before had been quietly growing at the summit of the cliff—and every other detail of that extensive field of ruin, were seen immediately in all their distinctness. Full in the midst of what appeared the highest part of the expanding mass, while yet in rapid motion, my attention was attracted by a tumultuous and somewhat upward-swelling motion of the earth, whence I fully expected to see burst forth a volume of pitchy smoke, and from which my present impression is, that gas, *purified from carbonaceous matter in passing through innumerable fissures of cold and damp material*, was still in progress of escape; but whether so or not, the remark made at the moment is sufficient to prove the absence of any impediment to distinct vision.

As regards the amount of tremor perceived, I must confess having speculated with some little anxiety on the probable stability of the abrupt and precipitous ridge on which I stood; and might therefore have somewhat underrated the exceedingly trifling movement which actually reached that point, and which I think I have felt surpassed by a heavy wagon passing along a paved street. The impression, slight as it was, was single and brief, and must have originated with the first shock of the powder, and not from the subsequent and prolonged rush of the ruins, which I can positively say communicated no perceptible tremor whatever.

I have not heard of a single scattered fragment flying out *as a projectile*, in any direction—and altogether the whole phenomenon was totally unlike anything which, according to ordinary ideas, could have been supposed to arise from the action of gunpowder. Strange as it may seem, this contrast between the actual and the expected effects, gave to the whole scene a character rather of sublime composure than of headlong violence, of graceful ease than of struggling effort. How quietly, in short, the gigantic power employed performed its work may be gathered from the fact that the operators themselves who discharged the batteries were not aware that they had taken effect, but thought the whole affair a failure, until reassured by the shout which hailed its success.

The remarkable absence of noise and tremor which characterized

* It has been stated that the flagstaff continued erect, but this (if I can credit the distinct evidence of my own senses) is incorrect.

this operation is explained by the structure of chalk as a material, and by the rifty state of the cliff as a body. Of all substances, perhaps chalk is the worst adapted for conveying sound, and the best for deadening the vibration propagated through it by a heavy blow. The initial hammerlike impulse of the newly-created gas on the walls of the chambers of the mines (of which it must be recollected there were *three*, simultaneously exploded), was doubtless thus deadened by traversing at least 75 feet of chalk, even in the shortest direction, or line of least resistance—and *this* must have taken place before the mass could have sensibly moved from its seat by the expansive force generated, which, however vast, proved incapable (as indeed it was expressly provided it should be) to communicate to its enormous load any greater velocity than barely sufficient to rift and bulge it outwards, leaving gravity to do the rest. Nothing can place in a more signal light the exactness of calculation which (basing itself on a remarkably simple rule, the result of long practical experience,) could enable the eminent engineer (Mr. Cubitt), by whom the whole arrangements are understood to have been made, so completely to task to its utmost every pound of powder employed, as to exhaust its whole effort in useful work—leaving no superfluous power to be wasted in the production of useless uproar or mischievous dispersion, and thus saving at a blow not less than 7,000£. to the company.

*Collingwood, Jan.* 31, 1843. J. F. W. Herschel.

Lond. Athenæum.

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*On the Progress effected in the Process of Gilding by the Electro-Chemical Method.* By Professor A. De La Rive.*

I shall unite under this title the account given of the progress of this application of science to the arts since I made it known in April, 1840.

Different artists have been occupied on it at Geneva, particularly M. M. Bergem, father and son, who have presented the Academy of Sciences at Paris, in the spring of 1840, objects gilded, and which after having been submitted to decisive proofs, have been acknowledged as nothing inferior in any respect, either in solidity or brilliancy, to the best gilding produced by the mercurial method. These gentlemen have brought to the process which I have described some perfections which they have not made known. This mode of gilding appears to meet with some obstacles in its application to pieces of brass for the movements of clocks and watches, because the color which it gives them is not such as is usually given to this species of work.

M. Droin, of Geneva, a distinguished workman, who is employed with M. M. Baute & Co., has succeeded, by following to the letter the process which I have described, in producing beautiful specimens of gilding; but he has remarked, that in order to succeed it is necessary to have the metal that we want to gild (silver or brass) perfectly homogeneous, a quality which it is difficult at all times to meet with.

* Archieves De L'Electricitie, No. 1.

The gilding will not be perfectly uniform except under this condition.

M. Perrot, of Rouen, has sent to the Academy of Sciences of Paris many samples of different metals which he has gilded by voltaic currents, and which appear to have succeeded very well.

M. Arago likewise presented the Academy of Sciences, in its session of the 5th of May last, with the spring of a chronometer from the Manufactory of Dent, gilded to great perfection by means of the galvanic process. He called to mind, on this occasion, that he had presented to the Academy a multitude of objects in metal, gilded by means of the same process by M. Perrot. M. Perrot had already at this time, also, springs to exhibit, which were gilded in the same manner, and if they were not comprised in the number of objects which he sent, it was because he was awaiting the completion of an experiment in which he had undertaken to gild, at the same time, all the movements of a watch; to gild them not only whilst in their place, but whilst performing their usual functions.

In Germany, a distinguished artist of Stuttgard, Mr. Reinecker, who is the author of a very remarkable process of waving or watering, has brought the process of gilding by the galvanic method to that point of perfection which, under the relation of solidity, leaves nothing to be desired. As to its beauty, it leaves far in the rear every other species of gilding. "If we consider," adds the *Gazette* of Stuttgard (from whence we draw these details), "that in the process of gilding by galvanism there is no disengagement of those mercurial vapors so hurtful to the operator, that the quantity of gold employed is much less than in the ordinary gilding, and that the greater part of the technical portion of the work is so simple that it may be executed by children, this discovery in this branch of industry will then appear of such importance that we can but express our ardent desire to see it generally employed."

M. Boettiger, in the *Annalen der Chemie und der Pharmacie* (vol. xxxv., p. 350), describes the efforts which he has made to gild and platinize the plates of copper in relief which have been obtained by the process galvanoplastic.

He tried, at first, to platinize them by the galvanic method, making use of a solution of choride of platina very neuter and very weak. He has succeeded, but he has found that it was necessary to have six immersions in the chloride of platinum to produce a covering of platinum of very small thickness; and again, this covering had not a very good color, but was rather grey than white.

Having found, by succeeding experiments, that for gilding no salt is preferable to the double chloride of gold and sodium, M. B. endeavored to employ, in platinizing, the double chloride of platinum and sodium. The experiments which he has thus made have perfectly succeeded. In most cases he did not find it necessary to give more than three immersions to re-cover with a sufficiently strong layer of platinum the largest surface of copper; the color also appeared of a much purer white. There is one circumstance, however, to which it is always necessary to pay attention in platinizing copper and other

metals, which is, strongly to rub the object with a small piece of linen, and to scour it immediately with chalk each time, without exception, that it is withdrawn from the solution of platinum. It is worthy of remark also, that whether in silvering, in gilding, or in platinizing, those objects which have the highest polish are those also which receive the metallic layer with the greatest facility and beauty. Mr. Boettiger thinks, that in order to produce good gilding on copper, it is necessary to commence by covering it with a cuticle of platinum, which is easy, and of very little expense; the gilding is far more beautiful and more durable than that obtained by the direct gilding of the copper. It is easy to obtain the double chloride of platinum and sodium by mixing equal parts of dry chloride of platinum and of pure common salt in distilled water. It is very necessary also to rub the metal with fine sand, moistened with hydrochloric acid mixed with chalk, in such a manner as to leave no traces of the oxide of copper, for platinum will not be precipitated on those places where any of it remains, and it will suffice to rub again those places, in order to determine this deposit of platinum, when the plate of copper is again put into the solution as the negative metal of the pair.

I cannot avoid remarking, that I have urgently insisted, in my notice, on the necessity of well cleansing the surface of the copper from verdigris before putting it into the solution of gold, and on the importance of successive immersions, and of the rubbing being repeated after each immersion with a fine linen cloth.

M. Snaer, in a work on electro-metallurgy, of which we shall give an account in our next number, employs for gilding voltaic currents far more powerful than those which I have indicated, and which have been generally used. He is said thus to obtain gilding much more solid, and of any thickness, as great as may be required.

He remarks that the process called the English one, of Elphinstone, does not offer this last advantage. In this process the object to be gilded, which is brass, for example, is put into a solution of double chloride of gold and sodium, raised to a high temperature. The gold is precipitated by the effect of the solution of a part of the metal equivalent to that on which the precipitation takes place. Now, as soon as all the surface is re-covered by a thin cuticle of gold, there is no longer a possibility of any part of this surface being dissolved, and thus the gold is no longer precipitated. It can only form, then, a very superficial gilding, and consequently of short duration; whilst, by the voltaic process there is no limit to the thickness which may be given to the lamina of gold. Another inconvenience of the English process is, that there ensues, by the dissolution of a part of the surface to be gilded, an alteration of that surface, a circumstance which proves prejudicial in many cases, especially when it acts on objects the dimensions of which have been very exactly calculated, and ought not be altered, such as the wheels of chronometers, for example.

M. Hammom, an engraver at Geneva, has found great advantage in the engraving by aquafortis, by substituting for the varnish which is made to cover the plate of copper, a cuticle of gold precipitated by voltaic agency: the tracings of the needle are far more delicate.

The lamina of gold being permanent, and not being broken or carried off like the coating of varnish; when the aquafortis has operated, we may with great facility correct the engraven plate if it has any defect, which is impossible in the old process. We shall give to our readers, in one of our next numbers, a specimen of this species of engraving, by means of which we have produced a new apparatus which we purpose describing on a future occasion. An. Elec. Mag. & Chem.

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## *On the Purification of Fish Oil. By* MM. GIRARDIN *and* PREISSER.

Translated for the Journal of the Franklin Institute, from the "Bulletin de la Societé d'Encouragement," for July, 1842.

The constantly increasing price of seed oils (huiles de graines) has drawn the attention of speculators to whale oil; and those who first thought of mingling the latter with vegetable oils, for purposes of illumination, have realized large profits. It is now difficult to find the oils of colza, &c., entirely free from fish oil.

In various scientific and technical works, we find processes for the purification of fish oils, which, although simple, are useless, and rather tend to mislead those engaged in their sale, or purification.

Thus, Mr. Davidson, of Edinburgh, purifies oil by treating it with one per cent. of chloride of lime, diluted with water, under violent agitation; and he assures us that the odor is entirely destroyed, but we obtain only a bleached and thick matter, which is clarified by adding 85 grammes of sulphuric acid, diluted with sixteen or twenty times its weight of water. The mixture is stirred, gently boiled, and, after filtering warm, is suffered to cool and repose for several days. MM. Girardin and Preisser repeated this process without any satisfactory result.

The "Journal hebdomadaire des Arts et Metiers" points out several processes for the same purpose. The first consists in mingling 28 grm. pulverized chalk, and 42 grm. slaked lime, with a gallon of the oil, stirring well, and adding 0.236 litre water; after two or three hours of repose, it is mixed again, and this operation repeated for two or three days; 28 grm. of common salt, dissolved in 0.710 litre water, is then added, the mixture stirred at intervals for two days, suffered to settle, and the oil drawn off.

Another process in the cold, applicable to cod oil, consists in putting into 4½ litres of the oil, previously prepared by the preceding process, 28 grm. of chalk; then, after 24 hours, 28 grm. of potassa, dissolved in 113 grm. water; and finally, after several hours, 57 grm. common salt, dissolved in 473 grm. water. After settling a few days, the oil is drawn off.

Neither of these processes is sufficient, as MM. G. and P. have satisfactorily ascertained. The same journal asserts that the oil is obtained so pure by the following process, that it can be employed in woolen manufactures.

Put into 4½ litres (1 gallon) of impure oil, 35 grm. chalk, an equal amount of slaked lime, and 0.473 litres of water; after stirring, and a

repose of several days, add 0.473 litres water, and 85 grm. potassa; heat the liquid, without bringing it to boiling, and draw it off when the oil has a light amber color; it has now only a pungent, fatty odor: finally, add 0.473 litres water, containing 28 grm. salt, and, after boiling the mixture for half an hour, turn off the oil into a reservoir. This process does not refine the oil.

Many English patents for the same purpose were tested by Messrs. G. and P.

One treats fish oils in the cold by bone black, in small fragments, and filters through animal charcoal, after repeated agitation. Such a process clarifies the oils, and removes a portion of their empyreumatic odor, but does not in the least diminish their essential odor.

Another method, recently published in France, has succeeded no better. It consists in pouring into the oil a solution of bichromate of potassa, mixing thoroughly, then adding a solution of oxalic acid; the action is energetic, but, after repose and drawing off, the oil still retains its characteristic odor.

There is a process among the French patents, which consists in heating the oil merely to simmering with ten parts of water for five or six hours, and, towards the close of heating, adding a milk of one part of water, with one-twelfth of chalk, and one-twelfth of lime. After settling perfectly, it is drawn off and run into reservoirs, through carded wool, or pounded charcoal. This process clarifies the oils, but decolorizes them imperfectly, and does not at all remove their odor.

At Rouen, they refine whale oil by sulphuric acid, as in operating on seed oils; but this method removes neither color nor odor. If, previous to this operation, it be stirred for some hours with chalk, and a current of steam be passed through it, a bleached liquid is obtained, which, by the addition of a suitable quantity of sulphuric acid, deposites plaster on settling. The clear oil, filtered through animal black, has lost a portion of its deep color, and has not a strong odor; but it is not perfectly purified, even after many successive filtrations.

The oxygenation of oils leads to very bad results. Messrs. G. and P. remark, that oils filtered and treated, whether by chlorides, lime, chalk, or animal charcoal, and then left to themselves for thirty or forty days, deposite a bleached organic substance, soluble in water and ether, analogous to margarine, and, while depositing, the oil is more and more decolorized. Fish oil may be obtained, of a quality resembling fine olive oil in appearance, by exposing it to the sun, then to the action of chloride of lime, and filtering several times through animal charcoal. The odor is lessened, but not entirely removed.

A simple exposure to the sun for several months determines an abundant deposite, while the oil is clarified, and sensibly purified.

If whale oil be brought in contact with caustic ley, employed cold, and in small quantities, the decolorization is hastened; the mass separates into two distinct strata—the upper one, decolorized, is very fluid and limpid, but always odorous; the lower, which is very small, is a mixture of the alkaline solution, strongly colored brown, and of all the solid portion of whale oil analogous to margarine. It is not

necessary to submit the decanted oil to any other process of purification; in this state it is suitable for all manufacturing purposes, excepting on account of its odor, which is always well defined.

It appears from the experiments of Messrs. G. and P. on fish oils, that we at present possess no sufficiently efficacious means of removing their strong and disagreeable odor. The best method, at present, is to submit them either to the action of alkalies, or to the successive action of chalk, steam, and sulphuric acid; to suffer them to repose, and filter several times through animal charcoal. We thus obtain a clear oil, less colored, and of a less repugnant odor; but its want of odor is out of the question.

The refining and purification of fish oils is the more important, since, for the last twelve years, their importation has constantly increased. Thus, in 1827, there was entered only 3,000,000 kilogrammes, (about 6,000,000 lbs.,) the greater part of which came from the islands of St. Pierre and Miquelon; while, in 1839, the importations amounted to 9,200,000 kilogrammes, representing a value of 5,500,000 francs.

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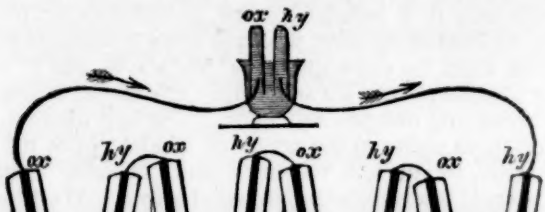
*On a Gaseous Voltaic Battery. By* W. R. Grove, Esq., M. A., F. R. S., *Professor of Experimental Philosophy in the London Institution.*

In the Philosophical Magazine for February, 1839, I have given an account of an experiment in which a galvanometer was permanently deflected, when connected with two strips of platina, covered by tubes containing oxygen and hydrogen. At the conclusion of my notice I say, "I hope, by repeating this experiment in series, to effect decomposition of water by means of its composition." The next paper of mine, published in the same year, contains an account of a battery to which the public has since attached my name, and which led me into a different field of research.

In reading over my papers lately, for a purpose alluded to in my letter of last month, I was struck with the above sentence. My impression was, that I had expressed a hope not very likely to be realized; but, after a few days' consideration, I saw my way more clearly, and determined to try the experiment.

As the chemical, or catalytic, action in the experiment detailed in that paper, could only be supposed to take place, with ordinary platina foil, at the line, or water-mark, where the liquid, gas, and platina met, the chief difficulty was to obtain anything like a notable surface of action. To effect this, my first thought was to surround the platina foil with spongy platina, precipitated in the usual way by muriate of ammonia. This was suggested to me by the known action of spongy platina on mixed gas, which would, by its capillary attraction, expose a considerable surface of metal and liquid to the action of the gases. I still think this would be the best mode of effecting the object; but, as it was very troublesome in manipulation, I determined to try the platina platinized by voltaic deposition from the chloride,




as proposed for a different purpose by Mr. Smee. I therefore caused a series of fifty pairs to be constructed, the form and arrangement of which is given in the annexed figure, where *ox* denotes a tube filled with oxygen; *hy*, one filled with hydrogen; and the dark line in the axis of the tube platinized platina foil, which, in the battery I constructed, was about one-fourth of an inch wide. It is obvious that, by allowing the platina to touch the liquid, the latter would spread over its surface by capillary action, and expose an extended superficies to the gaseous atmosphere. The battery was charged with dilute sulphuric acid, sp. gr. 1.2, and the following effects were produced:

1st. A shock was given which could be felt by five persons joining hands, and which, when taken by a single person, was painful.

2d. The needle of a galvanometer was whirled round, and stood at about 60°; with one person interposed in the circuit, it stood at 40°, and was slightly deflected when two were interposed.

3d. A brilliant spark, visible in broad daylight, was given between charcoal points.

4th. Iodide of potassium, hydrochloric acid, and water acidulated with sulphuric acid, were severally decomposed; the gas from the decomposed water was eliminated in sufficient quantity to be collected and detonated. The gases were evolved in the direction denoted in the figure, *i. e.*, as the chemical theory and experience would indicate, the hydrogen traveling in one direction throughout the circuit, and the oxygen in the reverse. It was found that 26 pairs were the smallest number which would decompose water, but that four pairs would decompose iodide of potassium.

5th. A gold leaf electroscope was notably affected.

6th. The battery was charged with distilled water; the electroscope was affected, and iodide of potassium decomposed.

7th. Although the phenomena were too marked to render it in the least probable that accidental circumstances could have produced the current, still counter experiments were carefully gone through; thus

the gases were repeatedly changed, oxygen being placed in the tubes which had contained hydrogen, and *vice versa*. The effects were equally powerful, and the direction of the current was reversed.

8th. All the tubes were charged with atmospheric air; no effect was produced.

9th. The battery was charged with carbonic acid and nitrogen in the alternate tubes; not the slightest effect observable.

10th. It was charged with oxygen and nitrogen; not any effect.

11th. With hydrogen and nitrogen, slight effects. The difference between this and the last experiment at first struck me as extraordinary, but, upon consideration, was easily explicable. The liquid, being exposed to the air, would necessarily absorb some oxygen, and this, with hydrogen, would give rise to a current. This was proved by the liquid rising in the hydrogen tubes, but not in those containing nitrogen; and, as a further proof, one set of tubes was charged with hydrogen, and the alternate set with acidulated water without gas; a slight current was perceptible: with oxygen and the liquid in alternate tubes, there were no effects produced.

12th. As the oxygen and hydrogen were procured in the first instance by electrolysis, and as Dr. Schœnbein, in his careful experiments on polarized electrodes, supposed the peculiar substance which he has named Ozone to be a principal agent, I caused the tubes to be charged with oxygen evolved from chlorate of potash and oxide of manganese, and hydrogen from zinc and sulphuric acid; the effects were the same.

The tubes were not all of equal size, nor were they graduated; the exact proportional diminution of gas in each tube could not be ascertained with perfect accuracy; both gases did diminish, and the hydrogen so much more rapidly than the oxygen, that my assistant, who was unacquainted with the rationale of the battery, observed that the hydrogen was absorbed twice as fast as the oxygen. Mr. Gassiot is now preparing a graduated battery of this sort, by which the point will be accurately determined; supposing the gases at the electrodes and the plates exposed to uniform facilities of solution, the quantity evolved should be equal to that absorbed.

Several curious points are suggested by this novel battery.

*a.* How is its action explicable on the contact theory? I am by no means wedded to any theory, and have constantly endeavored to look with the eye of a contact theorist upon the facts of voltaic electricity, but I cannot see them in that light; if there be any truth in the contact theory, I either misunderstand it, or my mind is unconsciously biased. Where is the contact in this experiment, if not every where? Is it at the points of junction of the liquid, gas, and platina? If so, it is there that the chemical action takes place; and, as contact is always necessary for chemical action, all chemistry may be referred to contact, or, upon the theory of an universal plenum, all natural phenomena may be referred to it. Contact may be necessary, but how can it stand in the relation of a cause, or of a force?

*b.* Its phenomena present to my mind a resolution of catalysis into voltaic force, in other words, the action of this battery bears the same

relation to the phenomena of catalysis as that of the ordinary batteries does to those of ordinary chemistry. Whether these effects could be produced by other inoxidable metals, (such as gold or silver,) is an experiment worth trying. The more we examine chemical and voltaic actions, the more closely do we assimilate them. For some mysterious reason, three elements seem necessary for very many, if not for all, chemical actions.

*c.* This battery is peculiar in having the current generated by gases, and by synthesis of an equal but opposite kind at both anode and cathode; it is, therefore, theoretically, more perfect than any other form, as the batteries at present known act by one affinity at the anode, and have to overcome another at the cathode.

*d.* This battery establishes that gases, in combining and acquiring a liquid form, evolve sufficient force to decompose a similar liquid, and cause it to acquire a gaseous form. This is, to my mind, the most interesting effect of the battery; it exhibits such a beautiful instance of the correlation of natural forces.

Many other notions crowd upon my mind, but I have occupied sufficient space, and must leave them for the present, hoping that other experimenters will think the subject worth pursuing.

Lond. & Edinb. Philos. Mag.

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FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

## *Facts and Observations on the Explosion of the Boiler of the Steamboat Medora. From a communication made by* CHARLES REEDER, *Engineer.*

The Committee on Publications have received some further facts relative to this explosion, from Mr. Reeder, of Baltimore,—the engineer to whom was intrusted the repair of the machinery of the unfortunate Medora, after the fatal disaster which happened to her upon the 14th of April, 1842, and which has already been described in a memoir by B. H. Latrobe, Civil Engineer, inserted in our last November number.

Mr. Reeder cut from the cylinder part of the exploded boiler, several strips of the metal, and subjected them to experiment, with the view of determining the absolute strength of the iron of which that boiler was constructed.

The following tabular statement furnishes the results of these experiments; the sixth trial was upon a strip of No. 3 wrought iron boiler plate, from the manufactory of Brooks & Co., of Pennsylvania; the other five trials were all made upon the iron of the Medora's ruptured boiler.

Since the mean strength of wrought iron boiler plate is about 55,000 lbs. per square inch of section, it is evident from Mr. Reeder's experiments, as recorded in the subjoined table, that the iron of the Medora's boiler must have been *of an inferior quality.*

| No. of the exper'ts. | Dimensions of the strip tried. Br'dth in inch. | Thick. in inch. | Area of section in sq. inch s. | Direction of strip in the sheet. | Weight which broke the strip in lbs. | Strength in lbs. pr sq. inch of sect'n | Remarks. |
|---|---|---|---|---|---|---|---|
| 1 | .250 | .237 | .05925 | across | 1720 | 29029 | broke in two minutes. |
| 2 | " | " | " | lengthwse | 3040 | 51308 | broke instantly. |
| 3 | " | " | " | " | 2625 | 44304 | do. |
| 4 | " | " | " | " | 2420 | 40844 | broke in three minutes. |
| 5 | " | " | " | " | 2440 | 41181 | do. |
| 6 | — | — | .06500 | " | 3780 | 58154 | — |

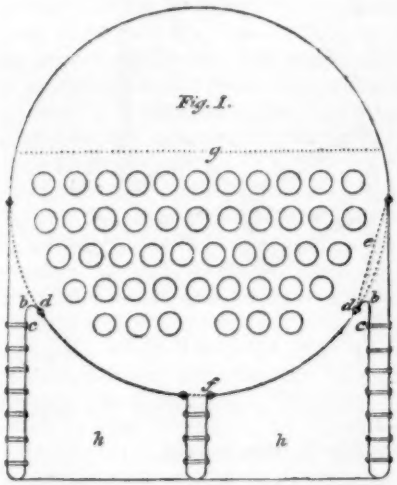


*Fig. 1.*

Mr. Reeder has furnished us with two sketches, (Figs. 1 and 2,) which we subjoin, as they exhibit more clearly the internal construction of the exploded boiler of the Medora, than those we have already published.

Fig. 1 is a transverse section of the boiler, in which *g* is the water line, above the return flues, and *h h* the places of the furnaces which heat the water.

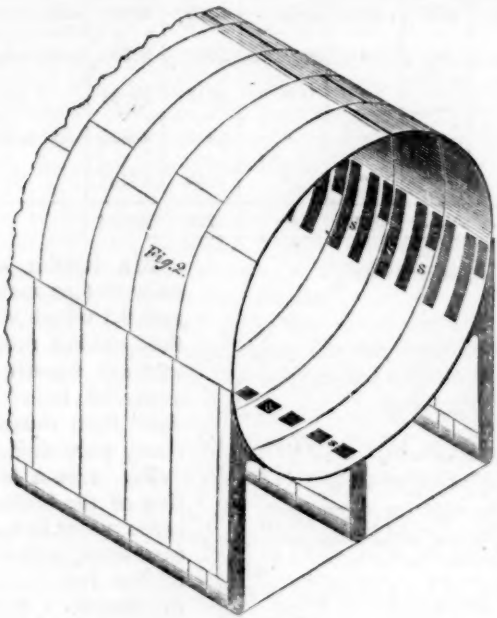
Figure 2 is an isometrical sketch, showing the interior of the boiler, and the large openings communicating with the water legs.

Mr. Reeder states that the length of the boiler, between the back and front flue heads, was 198 inches; the apertures, or parts cut away to form the openings of communication with the water legs, 117 inches.

Hence the aggregate breadths of the strips *s*, *s*, Fig. 2, amounted to but 81 inches, measured in the direction of the length of the boiler; but even this breadth of metal was again reduced by the rivet holes, which were $1\frac{1}{2}$ inches from centre to centre, and 46 in number, each being $\frac{11}{16}$ths of an inch in diameter; therefore $198 - (117 + 46 \times \frac{11}{16})$, or $198 - (117 + 31\frac{6}{16}) = 49\frac{4}{16}$ inches, the aggregate breadth of iron left, to resist the internal strain over the side water legs.

The ratio of the iron left, to the whole length of the boiler, is, consequently, $49\frac{4}{16} : 198$, or $1 : 4.08$; so that every 1.02 inches of the length of the boiler would require, from the average of the whole five experiments given above, a force of say 2449 lbs. to rend it asunder, or only about 2400 lbs. per lineal inch of one side.

But, taking the mean longitudinal strength of the strips from the boiler, tried by Mr. Reeder, and rejecting the first experiment, we have near 44,000 lbs. per square inch for the average strength of the iron.




Now, if, in the formula $x = \frac{2Pt}{D}$, where $x$ represents the pressure per square inch, we assume, from Mr. Reeder's data,

$$D = 132 \text{ inches};$$
$$P = 44{,}000 \text{ lbs.};$$
$$t = .237 \text{ of an inch};$$

then we have $x = \frac{2 \times 44{,}000 \times .237}{132} = 158$ lbs. per square inch of steam pressure, required to burst the Medora's boiler, if it were a continuous hollow cylinder, and the circumferential strength be alone regarded; but as the metal cut away over the side water legs amounted to $148\frac{4}{16}$ inches, out of 198 inches of the boiler's length, leaving, in fact, but $49\frac{4}{16}$ lineal inches of iron, $\frac{237}{1000}$ of an inch thick, to resist the circumferential pressure within, we have then the following proportion— $198 : 49\frac{4}{16} :: 158 : 39\frac{4}{16}$ lbs.

Therefore, from the data furnished by Mr. Reeder, it seems apparent that the maximum circumferential strain which the boiler of the Medora could have borne without bursting, *did not exceed* $39\frac{4}{16}$ *lbs. per square inch!*

With regard to the steam pressure which the fully loaded safety valve could fairly impose upon the boiler in question, Mr. Reeder states that the large weight, which our intelligent correspondent, Mr. Latrobe, estimated to weigh 200 lbs., did actually weigh 210 lbs.; and Mr. R. being of opinion that the pressure on a safety valve ought to be calculated only by the steam pressure upon its *lower* surface, he estimates the load of that safety valve thus:—266 lbs. of weights × 16.128 leverage = 4290.048; to which the weight of the valve, &c., &c., as estimated by Mr. Latrobe, (692 lbs.) being added, we have $\frac{4290.048 + 692}{182.7} = 27\frac{26}{100}$ lbs. pressure of steam per square inch, necessary to raise the safety valve, if perfectly free of adherence to its seat.

Mr. Reeder expresses the opinion that the main defects of the Medora's boiler were, firstly, that too much metal was cut away over the water legs, (*a a*, Fig. 2,) without in any manner bracing the apertures, or substituting the strength so removed; secondly, that the inside sheets of the side legs were joined to the cylinder, (as at *b d*, Fig. 1,) *by being curved downwards*, instead of being continued vertically upwards to a junction; and, lastly, that the flues were so disposed as to prevent the insertion of braces from the top, to support the bottom of the cylinder.

Mr. Reeder further remarks, that the strips *s*, *s*, being 33 inches long, of the same curvature as the cylinder, and subject to be drawn upon both sides, in a straight direction, (*d*, *e*, Fig. 1,) by the steam pressure, formed, in fact, a series of springs, upon which the lower half of the boiler was hung, or supported, so that, when the full steam pressure was on, the several strips would be drawn nearly into right lines, and, when it was off, they would spring back, and recover their curvature—thus allowing the boiler to vary essentially, in its shape, whenever put into use. From the observations which he made, and which he had so good an opportunity of making, Mr. Reeder appears to think that the boiler of the Medora was, in all probability, injured, or weakened, in the two trials which were made with it, prior to the 14th of April; and he expresses the decided opinion, that 23 lbs. pressure per square inch *was enough* to produce, upon the last trial, the fatal explosion which at that date took place; and Mr. Reeder also expresses a belief that this boiler would have *eventually* failed, under a pressure not exceeding 15 lbs. to the square inch.

Mr. R. further observes, that the evidence "of John Watchman, (the builder of the Medora's engine and boiler,) who tried (unsuccessfully) to raise the safety valve, when the steam gauge showed ten inches of steam, might impress some persons with the belief of its being fastened down."

"His mode of raising the valve was by a small cord attached to the end of the lever, and running over a small pulley directly above it, thence to a similar pulley, and finally down to a place convenient to the engineer."

"Under a pressure of 10 lbs. by the gauge, if the weights were out to, or near, the end of the lever, a man would have to overcome a

weight of about 170 lbs., (in addition to the friction of those pulleys,) by pulling down upon the cord."

"Now, the reason that he could not lift the safety valve, was not because it was fastened down, but simply because, being of less weight himself, he was not able to overcome that which he had to lift."

We will now conclude this subject by remarking, that the mystery which, in the opinion of Mr. Latrobe, seemed to hang over the causes of the explosion of the Medora's boiler, appears to be wholly removed, or cleared up, by the information derived from Mr. Reeder; for, as the steam pressure imposed by the weighted valve amounted, at the least, to 27 lbs. per square inch, whilst the maximum strength of the boiler was only 39 lbs., or but little more than 40 per cent., over the clear circumferential strain, even if we leave the longitudinal stress out of view, there was evidently not sufficient surplus strength to compensate for the imperfections in materials and workmanship, which are inseparable from such constructions, *and which always render a wide margin necessary between the maximum strength and the maximum pressure*—a margin, which a just regard to safety absolutely requires to be such, that the original strength of a steam boiler shall be, at the least, equal to treble the pressure to be ever imposed.

It would therefore seem to be a safe conclusion, that, in seeking for the causes of the explosion of the steamer Medora, *we need not go beyond the insufficient strength and injudicious construction of the boiler, to find a sufficient reason.*

We learn that Mr. Reeder has reconstructed the boiler of this steamboat upon the same general outline, but he has carefully avoided its imperfections; thus, he has placed the flues in regular order above each other, so as to admit of the insertion of stay bars, or braces, both vertically and horizontally; and alongside of the openings into the water legs, which occupy, in the aggregate, much less space than in the old boiler, he has secured broad bars of iron, in such manner as to substitute, or, in effect, to replace, in point of strength, the sectional area of iron removed; finally, he has continued the inner sheets of the side water legs vertically upwards to join the cylinder, and has formed the connecting flanch *internally*.

With these judicious alterations, and the great additional strength thereby imparted to the boiler, the Medora, under the new name of the *Herald*, is now, and for some time has been, working safely and successfully in the Chesapeake Bay.

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### *On the Blowpipe.* By Theo. F. Moss, *Mining Engineer.*

The importance of the Blowpipe in analytical research is daily advancing with the progress of chemistry. The knowledge of its use is of the utmost importance to the mineralogist, for with the assistance of a few simple re-agents, he is enabled to determine in a few minutes most of the compounds of a mineral, which by the usual

process of analytical examination, in the wet way, would require hours and often days. When traveling, he is often compelled to let his curiosity rest satisfied concerning the nature of minerals, which he may find, till he returns to his laboratory; whereas with a knowledge of the use of the blowpipe, he would carry with him the means of determining minerals and their component parts on the spot, and of ascertaining the presence of substances, which if contained in small quantities would be detected only by the most accurate analysis. An examination before the blowpipe is to be considered as preparatory to a quantative analysis by the wet-way.

It is not, however, meant that the blowpipe is infallible in detecting all the compounds of a substance, for one of its components may have such a strong re-action as to conceal the re-action of many others; nor is the following meant as a complete treatise on the blowpipe, but merely to give an insight into its importance, and some of its easier applications, which it is hoped may be of service to the Mineralogist in his summer excursions through our interesting country, and perhaps aid in adding the knowledge of the blowpipe to his other acquirements.

Supposing the reader to be acquainted with the form and manipulations of the blowpipe, I will proceed to describe the order of examination of a substance before the blowpipe and its different re-actions.

The quantity of mineral which serves for examination need be but small; for many operations a splinter the size of a millet seed will suffice, and for some even a much smaller quantity is more than sufficient; another advantage which the blowpipe possesses especially if we have to deal with valuable and rare specimens.

The first operation is to place a small portion of the substance in a thin glass tube closed at one end, then to heat it over a spirit lamp, and afterwards by the blowpipe flame. The object of this operation is to determine what volatile substance the mineral may contain, and sometimes to prepare it for succeeding operations. One of the volatile substances most commonly met with is water, either hygoscopic or chemically combined. The state in which it exists is easily determined by examining the deposite at the cool end of the tube, and ascertaining whether it has an alkaline or acid re-action. An acid re-action of the water results from the decomposition of acid salts, seldom from the decomposition of neutral salts, and then usually from the nitrates, which fill the glass tube with the vapors of nitrous acid, and from the sulphates when sulphurous acid is disengaged, which may be known by the smell.

Fluoric acid is also driven out of some of its compounds when water is present, and this is easily recognised by its pungent smell, and by its destroying the lustre of glass. Besides water, many substances when heated in a glass-tube closed at one end, are volatilized and deposited on the cooler parts of the tube; these sublimates are distinguished from one another by their color, fusibility and volatibility.

A sublimate of red-brown globules, which on cooling become yellow, denote sulphur. A reddish sublimate, or one which in large

quantities is black, and which on being rubbed has a dark red appearance, shews the presence of selenium. This burns in contact with the air, with a blue flame, spreading a very characteristic smell like decayed horse-radish.

Arsenic sublimes also when the substance contains metallic arsenic or any of the arseniurets, and also some of the arseniates, and is easily known from the vapors, having a smell like garlic.

Quicksilver is easily known, and forms a grey sublimate which on being moved forms into small globules of metallic quicksilver.

Heat sublimes cadmium from most of its compounds, and the sublimate when heated in contact with the air is changed into a yellowish brown vapor of oxide of cadmium.

Tellurium sublimes at a strong red heat, and deposites on the cool end of the glass tube in metallic globules.

The oxide of antimony sublimes in shining needles, melting first into a yellow fluid.

The oxide of tellurium has a similar action, but is not so easily volatilized, and does not sublime in crystals.

Arsenious acid sublimes very easily.

Arsenic acid is changed by a high heat into arsenious acid, and gives the same sublimate.

Osmic acid sublimes in white drops and crystaline needles, and disengages a very characteristic smell which attacks the eyes and nose in a very unpleasant manner.

The chloride of mercury gives a yellow sublimate, which on cooling is greyish white.

After having examined the mineral in a glass tube closed at one end, the next operation is to heat another portion of the substance in a tube open at both ends, the object of which is to see if volatile substances are disengaged by contact with the air; by inclining the tube more or less from a horizontal position, we have it in our power to increase or diminish the access of air. The substances which are thus disengaged, escape either as gases or are sublimed in the cooler end of the glass tube.

Sulphurous acid is one of the substances which escape in this manner as gases. The smallest quantity of this, which may be easily detected, if, when heating, the tube be held nearly horizontal, and then immediately brought in as near a perpendicular position without letting the substance drop out, and held with its upper end to the nose, when the sulphurous acid is easily detected by its pungent smell.

Combinations of selenium, tested in like manner, give a red sublimate of selenium and the peculiar smell of selenium.

Combinations of tellurium give a grey or greyish white sublimate, which melts into clear transparent globules.

Combinations of arsenic give when heated in the glass-tube open at both ends, a similar sublimate to the combinations of arsenious acid, when heated in the tube closed at one end.

The sulphuret of bismuth and the metallic combinations of bismuth give a sublimate of oxide of bismuth, which by heating, melts into

yellowish brown drops. Minerals containing a small quantity of bismuth give a sublimate which is surrounded by dark yellowish sublimate of bismuth, which becomes paler on heating.

Lead has a similar action, but the sublimate is much lighter.

Sulphuret of lead and selinuret of lead give a white sublimate, which however melts to a grey color.

The sulphuret of tin gives a white flaky sublimate.

Molybdanic acid, a white powdery sublimate and light yellow shining crystals, which are easily volatilized.

(To be continued.)

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### *Making Malleable Iron direct from the Ore.*

The President in the chair.—In the conversation which was renewed upon Mr. Pole's paper, 'On the Comparative Friction of Beam and Direct-action Steam Engines,' the author further explained the nature and objects of his paper, which had not been fully understood on the former evening, and illustrated the mode of analytical reasoning, by which he had arrived at his conclusions. He then proceeded to answer the objection which had been raised against the laws of friction adopted by him, and to comment upon the mode of experimenting of Vince and others; showing, on the other hand, by quotations from the recorded experiments of Amonton, Coulomb, Rennie, and Morin, and from the works of Gregory, Brewster, Emerson, Playfair, Barlow, Farey, De Pambour, Posson, Pratt, Whewell, and Moseley, that the views he had taken were correct. He also noticed the variations produced by attrition, and by the introduction of unctuous substances between the rubbing surfaces. These views were corroborated by several members present; some of whom had been quoted as authorities, and the propositions involved appeared to be generally received.

'A new mode of making malleable Iron direct from the ore at one process;' it is the invention of Mr. Clay, and is used at the Shiroa works, near Kirkintilloch. By this process a mixture of dry hæmatite, or other rich iron ore, is ground up fine, with about fourtenths of its weight of small coal; this mixture is allowed to pass gradually through a hopper into an oven adjoining, and forming part of a species of puddling furnace, into which a given quantity is drawn at stated times, when thoroughly and uniformly heated. The charge is then puddled in the usual manner, but with less labor than when working plate iron; and in about an hour and a half the iron is produced in a malleable state, fit for shingling and rolling into bars After another process of filing and rolling again, malleable iron bars are produced of a quality (as was stated by some members present) superior to the cable bolts or best iron usually made by the long and expensive process of calcining the ore, smelting in the blast furnace, and refining the pig-iron, and the saving of fuel is necessarily very great. The iron was stated, also, to be capable of being converted into steel of superior quality, and when worked by Mr. Heath's plan,

of uniting manganese in the process, cast steel was produced, which possessed the property of welding or uniting to iron; and, in consequence, all the cutlery which was formerly made of shear steel was now made of cast steel. The cast iron produced by the scoriæ, or refuse slag of this process, is of a better quality, in consequence of the absence of phosphoric acid, which is ordinarily present in the limestone, and as a flux in the blast furnace. This discovery may be the means of working the comparatively unopened mines of hæmatite of rich quality existing in Lancashire, Devonshire, and Cornwall, all of which could be brought into use by this means; and if, as asserted, the iron made good steel, England would be rendered independent of Sweden.

The discussion was renewed upon Mr. Clay's process of making malleable iron from hæmatite ore. It was shown that of the twenty-five thousand tons of steel made annually in this country, not more than two thousand five hundred tons were made from the best quality of Swedish iron; the rest was made from inferior charcoal iron from Russia and Germany, or from English iron, which was not well calculated for converting.—*Trans. Inst. Civ. Eng.*

Lond. Athenæum.

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## *Meteorological Observations for January*, 1843.

| Moon. | Days. | Therm. Sun Rise. | Therm. 2 P.M. | Barom. Sun Rise. | Barom. 2 P.M. | Wind. Direction. | Wind. Force. | Water Fallen in rain | State of the Weather, and Remarks. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 29° | 30° | 30.05 | 30.06 | W. | Moderate | | Clear. | Clear. |
| | 2 | 25 | 39 | 30.00 | 29.80 | E. S. | do | | Cloudy. | Lightly cloudy. |
| | 3 | 26 | 23 | 29.75 | 29.94 | W. | Brisk | | Clear. | Clear. |
| | 4 | 10 | 24 | 30.20 | 30.20 | W. | Moderate | | Cloudy. | Clear. |
| | 5 | 24 | 39 | 30.00 | 30.00 | S. | do | | Cloudy. | Cloudy. |
| | 6 | 30 | 41 | 30.00 | 30.10 | E. | do | | Cloudy. | Cloudy. |
| | 7 | 36 | 48 | 30.05 | 30.05 | E. | do | | Cloudy. | Cloudy. |
| ☾ | 8 | 51 | 59 | 29.90 | 29.80 | E. S. | Blustering | .29 | Cloudy. | Rain. |
| | 9 | 34 | 48 | 30.10 | 30.10 | W. NW. | Moderate | | Clear. | Clear. |
| | 10 | 41 | 58 | 30.30 | 29.95 | SE. | do | .60 | Fog. | Rain. |
| | 11 | 39 | 50 | 30.30 | 30.06 | N. | do | | Clear. | Cloudy. |
| | 12 | 39 | 49 | 29.85 | 29.85 | NW. | do | | Cloudy. | Cloudy. |
| | 13 | 40 | 43 | 29.60 | 29.60 | W. | do | .28 | Cloudy. | Rain. |
| | 14 | 36 | 32 | 29.70 | 29.75 | W. | Blustering | | Par. cloudy. | Clear. |
| | 15 | 27 | 36 | 30.00 | 30.00 | W. | Moderate | | Clear. | Clear. |
| ☉ | 16 | 32 | 43 | 30.30 | 30.30 | W. | do | | Clear. | Clear. |
| | 17 | 27 | 39 | 30.46 | 30.30 | E. | do | | Clear. | Clear. |
| | 18 | 29 | 46 | 30.30 | 29.34 | NW. | do | | Clear. | Clear. |
| | 19 | 32 | 51 | 30.30 | 30.20 | W. SW. | do | | Clear. | Hazy. |
| | 20 | 48 | 60 | 30.20 | 30.20 | W. | do | | Cloudy. | Par. cloudy. |
| | 21 | 46 | 57 | 30.05 | 29.76 | W. | do | | Cloudy. | Par. cloudy. |
| ☽ | 22 | 48 | 56 | 29.90 | 29.75 | W. | do | | Cloudy. | Clear. |
| | 23 | 34 | 45 | 29.75 | 29.50 | W. | do | | Clear. | Cloudy. |
| | 24 | 46 | 42 | 29.25 | 29.25 | W. | Blustering | | Flying clouds. | Flying clouds. |
| | 25 | 33 | 40 | 29.44 | 29.44 | W. | do | | Clear. | Flying clouds. |
| | 26 | 21 | 32 | 29.44 | 30.00 | NW. | Moderate | | Clear. | Clear. |
| | 27 | 25 | 36 | 30.05 | 30.05 | SE. | do | .18 | Cloudy. | Rain. |
| | 28 | 34 | 43 | 29.75 | 29.75 | W. | Brisk | | Drizzle. | Cloudy. |
| | 29 | 33 | 36 | 30.05 | 30.10 | NE. | Moderate | | Cloudy. | Clear. |
| ⊗ | 30 | 24 | 37 | 30.10 | 30.00 | NE. SE. | do | | Clear. | Hazy. |
| | 31 | 46 | 42 | 29.50 | 29.15 | SE. | do | .34 | Drizzle. | Rain. |
| | | 33.73 | 42.71 | 29.96 | 29.88 | | | 1.69 | | |

| Thermometer. | | Barometer. | |
|---|---|---|---|
| Maximum 60 on 20th. | Mean 38.22 | Max. 30.46 on 17th. | Mean 29.92 |
| Minimum 10 on 4th. | | Min. 29.15 on 31st. | |